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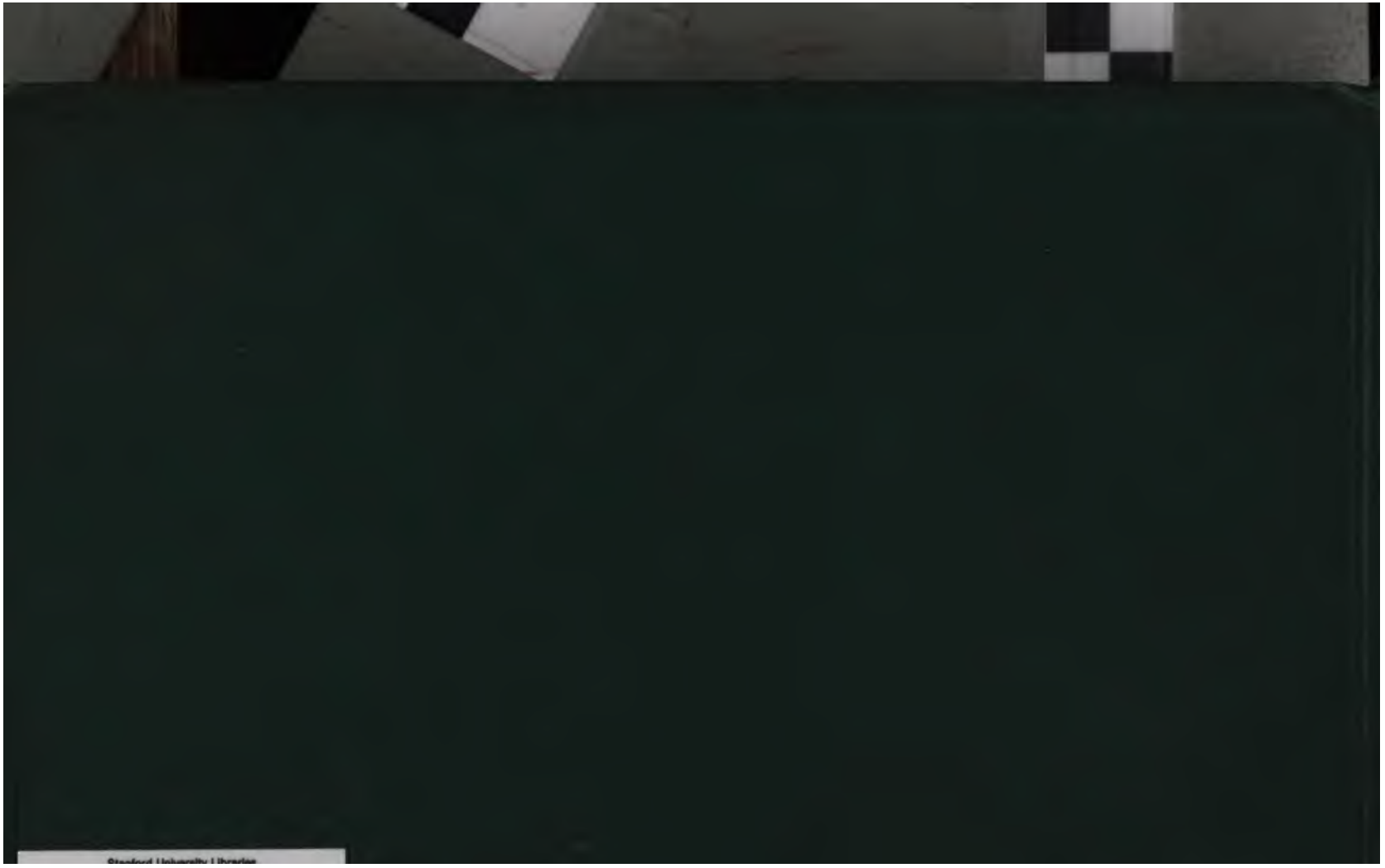
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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1880 *TO NOVEMBER* 1881.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLI. NOVEMBER 12, 1880. No. 1.

EDWIN DUNKIN, ESQ., F.R.S., Vice-President, in the Chair.

The Rev. F. B. Allison, M.A., Grammar School, Chesterfield;

Benjamin Francis Cobb, Esq., 79 Cornhill, E.C., and 148, Cornwall Road, W.; and

Henry Augustus Severn, Esq., 79 Earl's Court Road, Kensington, W., and Devalah, Madras, India,

were balloted for, and duly elected Fellows of the Society.

Comparison of the Observations of the Sun made at Washington, 1866–1875, with Hansen's Tables. By Dr. C. R. Powalky.

Explanation of the method used, and the meaning of the notation. Let $z_1, z_2 \ldots z_n$ be the arithmetical means of C.—O. for each month in the different years; $n_1, n_2 \ldots n_n$ the number of observations compared; Σ the sum of the squares of the residuals for each set; $w_1, w_2 \ldots w_n$ the weights; then for the first approximation Z is found by putting:—

$$w_1 = \frac{n_1(n_1-1)}{\Sigma_1 + n_1(Z-z_1)^2},\ w_2 = \frac{n_2(n_2-1)}{\Sigma_2 + n_2(Z-z_2)^2} \ldots .$$

$$Z = \frac{w_1 z_1 + w_2 z_2 + \ldots + w_n z_n}{w_1 + w_2 + \ldots + w_n}, \text{ and } W = w_1 + w_2 + \ldots w_n \text{ by } \textit{indirect} \text{ way.}$$

I. Declination.

Example for January.

Years.	Date.	n	z	Σ	Mean Error.
			"		"
1866	Jan. 8	5	+1·40	5·6	±0·28
1867	22	20	+0·57	23·9	±0·06
1869	23	8	−0·80	18·6	±0·33
1872	22	12	−1·60	40·8	±0·31
1873	26	2	−0·80	3·9	±1·96
1874	18	7	+0·67	35·7	±0·85
1875	18	5	+0·68	4·5	±0·22

Jan. 20·0 Z = +0″·45 W = 28·3

See the values of z, Σv^2 and n in 'Detailed Calculations for t Declinations.'

In this way the following results were found for the fi approximation (see details in the second approximation) :—

Jan. 20·0	Z = +0″·45	W = 28·3
Febr. 16·5	+0·03	37·8
Mar. 20·0	−0·17	57·5
Apr. 18·9	+0·22	96·0
May 17·4	+0·20	94·0
June 20·0	+0·29	73·0
July 17·0	+0·67	99·0
Aug. 14·6	+0·66	73·0
Sept. 17·3	+1·14	48·0
Oct. 17·1	+0·96	59·4
Nov. 13·7	+0·75	48·0
Dec. 13·6	+0·75	43·0

From these I formed twelve equations for determining :—

$d\phi'$ . . . . a constant, to be applied with contrary sign to the observati
($-d\phi'$ . . . . a correction of latitude or zenith-point assu
in the Volumes).

dl . . . . correction of the equinoctial point in the Solar Tables.

$d\omega$ . . . . „ „ obliquity of the ecliptic „ „

dM and de (M the longitude of perihelion, e excentricity of t

orbit) remain here indeterminate, to be determined further on by means of the Right Ascensions.

The solution gives:—

$$\left.\begin{array}{lll} dl = +1''.140 - 0.797\, d\mathrm{M} - 0.04\, de \ldots . & \mathrm{W} = & 61 \\ d\omega = +0.09 \ +0.001\, d\mathrm{M} + 0.03\, de \ldots . & & 321 \\ d\phi' = -0.489 + 0.000\, d\mathrm{M} - 0.11\, de \ldots . & & 663 \end{array}\right\} \text{II.}$$

N.B.—The value of de becomes very small and $d\mathrm{M}$ very uncertain, in consequence of the different systems of fundamental Star-places, 1866–1869 and 1870–1875, and the respectively small number of observations in these two intervals.

Assuming $d\mathrm{M}=0$, $de=0$, I found by computation from the above solution the values for z and the residuals (C.—O.), as in the following scheme:—

Jan. 20·0;	z (observed) =	$+0''.45$;	z (computed) =	$+0''.32$;	Resid. =	$+0''.13$
Feb. 16·5	„	+0·03	„	+0·14	„	−0·11
Mar. 20·0	„	−0·17	„	+0·04	„	−0·21

&c., &c.

Taking for *second approximation* the computed values of z from II. as the correct ones and determining anew their weights, then denoting the observed values by z and the computed corrections by z', I found now *directly* (as seen in the 'Detailed Calculation for the Declinations') these new values of z:—

Date.	z	z'	R.	W.
Jan. 20·4	$+0''.41$	$-0''.32$	+0·09	26·1
Feb. 16·4	+0·24	−0·14	+0·10	48·2
Mar. 20·0	−0·05	−0·04	−0·09	54·3
Apr. 19·2	+0·15	−0·06	+0·09	99·6
May 17·3	+0·21	−0·17	+0·04	95·7
June 20·1	+0·30	−0·41	−0·11	71·7
July 17·4	+0·67	−0·62	+0·05	97·1
Aug. 14·9	+0·68	−0·81	−0·13	74·3
Sept. 18·9	+1·04	−0·88	+0·16	39·2
Oct. 17·1	+0·94	−0·96	−0·02	60·9
Nov. 13·5	+0·76	−0·87	−0·11	47·2
Dec. 12·7	+0·60	−0·65	−0·05	43·1

I consider a new solution for a more accurate determination of $d\phi'$, dl, and $d\omega$ unnecessary, the values from the first solution, as shown by the residuals, agreeing quite satisfactorily.

As to the Declinations, I was compelled to adopt this method, viz., of combining the sets from arithmetical means of each year, because the elements of reduction (division and flexure of the circle) were altered each year. As to the Right Ascensions (compare *Astron. Nachr.* No. 2105):—

'An essential condition for determining the elliptical elements of the Sun's orbit is an accurate determination of the differences of R.A. of the Stars from which the R.A. of the Sun is derived.'

Here we have two systems of fundamental stars, one used for the R.A. of the Sun from 1866–1869, the other for the R.A. 1870–1875, which therefore we must treat separately. We have more observations of the Sun's R.A. from 1866 to 1869, and their successive series of sets agree far better than those from 1870 to 1875. I was at liberty to unite the differences (C.—O.) for periodical intervals of twenty-three consecutive days of the year, regardless of their occurrence within a single year, by arithmetical means; but I adopted the same method and process as with the Declinations—namely, that of approximation. Finally I saw that in regard to results it would be nearly indifferent which of the methods was employed; only in regard to weights (the unity of weight is here taken as $0^{s}\cdot1$) some difference is perceptible.

The solution for determining $\Delta a'$ (a constant correction of all fundamental Star-places), ΔM and $\Delta\omega$, shows the coefficients of $\Delta a'$ and ΔM, in consequence of the small number of observations in both systems, so nearly equal that, as $\Delta a'$, the value of ΔM, and consequently also the value of dl from declinations, however certain may be the numerical value in

$$\Delta l = +1\cdot140 - 0\cdot797\Delta M$$

fails to give an indisputable decision.*

II. Right Ascension, 1866–1869.

with corrections for perturbations by ♀, ♂, ♃, taking for m' (♀) Le Verrier's value, or $m'=0\cdot00000250$ (instead of $0\cdot00000245$), for m'' Hall's value, and for m^{IV} Bessel's; for nutation Peters-Nyren's.

Originally I intended to determine, besides the elements of the Sun's orbit, and especially the equinoctial point, the mass of ♀ from observations in Washington with the Transit-Circle from 1862–1880; but this would require a totally new reduction in R.A., founded on a single system of fundamental star-places.

The following comparisons in means, and the results from them, can therefore only be regarded as a preliminary task to show the insufficiency of the hitherto adopted reductions in R.A.

Denoting by s the value of $\Delta\alpha$ (C.—O.) in arc, from the

* The sentence is printed as it stands in the MS.; as are also the symbols d, Δ, and the accented and unaccented letters a, a', ϕ, ϕ'.—Ed.

method of approximation by weights (by combining arithmetical means for each year), by z', the values of Δa (C.—O.) from purely arithmetical mean, we find :—

1)	Jan. 14·0	$z = -0''.795$	W = 11·5	Jan. 12	$z' = -0''.585$	$n = 16$
2)	Jan. 30·7	−0·645	26	Feb. 3	−0·420	28
3)	Feb. 18·6	−0·540	46	„ 19	−0·135	31
4)	Mar. 16·1	−0·285	43	Mar. 15	−0·300	28
5)	Apr. 4·4	−0·150	69	Apr. 4	−0·060	32
6)	„ 28·3	−0·060	31	„ 28	+0·075	38
7)	May 21·1	−0·330	90	May 21	−0·450	39
8)	June 14·9	−0·495	65	June 15	−0·450	38
9)	July 9·4	−0·360	71	July 10	−0·300	39
10)	Aug. 1·0	−0·180	100	Aug. 1	−0·405	31
11)	„ 22·8	−0·450	46	„ 22	−0·450	30
12)	Sept. 12·5	−0·345	36	Sept. 13	−0·345	22
13)	Oct. 8·0	−0·120	133	Oct. 8	−0·150	27
14)	Nov. 0·7	−0·030	72	Nov. 1	−0·300	37
15)	„ 25·2	−0·825	38	„ 26	−0·825	25
16)	Dec. 18·4	−0·990	30	Dec. 18	−1·065	21

From the sixteen equations with z and W, I obtained :—

$$\left.\begin{aligned} \Delta M &= +1''.587 - 1\cdot005\,dl - 0\cdot22\,de \;\ldots\; W^* = 2\cdot9 \\ \Delta\omega &= +0\cdot105 + 0\cdot002\,dl + 0\cdot005\,de \;\ldots\; W = 429 \\ \Delta a' &= -1\cdot235 + 0\cdot015\,dl + 0\cdot23\,de \;\ldots\; W = 2\cdot4 \end{aligned}\right\} \text{III.}$$

By II. (Declin.) and III. (R.A.) connected,

$\Delta\phi = +0''.50$ (correction of zenith point)
$\Delta\omega = +0\cdot09$
$\Delta\varepsilon = +0\cdot10$

and very uncertain,

$\Delta M = +1''.23$
$\Delta l = +0\cdot15$
$\Delta a = +1\cdot23$ (correction of R.A. of comparison stars).

* Unity of W = 0''.1 = 1·5″.

Detailed Calculation for the Declinations.

Second Approximation. The Years 1866–1875 *denoted by* 1–10, *the Date of the Month by* t; $\Sigma w^2 + n(Z-z)^2$ *by* Σ'.

(January, $Z = +0''.32$, from computation in the first approximation.)

Year.	t	z	Σw^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	Logarithms z	Logarithms w	Logarithms t	Numbers zw	Numbers w	Numbers tw
1	8	$+1''.36$	5·6	5	1·04	5·4	1·301	1·041	0·134	0·260	0·903	+ 2·5	1·8	15
2	22	+0·57	23·9	20	0·25	1·3	2·580	1·401	9·756	1·179	1·342	+ 8·6	15·1	332
4	23	−0·79	18·6	8	1·11	9·9	1·748	1·455	9·898	0·293	1·362	− 1·6	2·0	45
7	22	−1·56	40·8	12	1·78	38·0	2·121	1·897	0·193	0·224	1·342	− 2·6	1·7	37
8	26	+0·80	3·9	2	0·48	0·5	0·301	0·643	9·903	9·658	1·415	+ 0·4	0·5	12
9	18	+0·67	35·7	7	0·35	0·9	1·623	1·564	9·826	0·059	1·255	+ 0·8	1·1	21
10	18	+0·68	4·5	5	0·36	0·7	1·301	0·712	9·833	0·589	1·255	+ 2·6	3·9	70
											Sum	+ 10·7	26·1	532
											Logar.	1·029	1·417	2·726

Jan. 20·4, $z = +0''.41$, $W = 26''.1$.

(February, Z = +0″·14)

Year.	t	z	Zw^0	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	$\mathfrak{M}$	s	Logarithms w	t	Numbers nw	w	tw
1	16	+0·70	12·5	15	0·56	4·7	2·322	1·235	9·845	1·087	1·204	+ 8·5	12·2	195
2	14	+0·32	11·2	11	0·18	0·4	2·041	1·065	9·505	0·976	1·146	+ 3·0	9·5	132
3	17	−0·17	22·7	8	0·31	0·8	1·748	1·371	9·230	0·377	1·230	− 0·4	2·4	40
4	19	+0·22	12·7	13	0·08	0·1	2·193	1·107	9·342	1·086	1·279	+ 2·7	12·2	232
5	17	−1·34	14·2	9	1·48	19·7	1·857	1·530	0·127	0·327	1·200	− 2·8	2·1	36
7	13	−0·50	1·8	4	0·64	1·6	1·079	0·532	9·699	0·547	1·114	− 1·8	3·5	46
8	10	{−1·1	—	1}	rejected									
9	18	−0·18	10·9	8	0·32	0·8	1·748	1·068	9·255	0·680	1·255	− 0·9	4·8	86
10	15	+1·01	16·1	6	0·87	4·5	1·477	1·314	0·004	0·163	1·176	+ 1·5	1·5	22
											Sum	+11·8	48·2	789
											Logar.	1·072	1·683	2·897

Febr. 16·4, s = +0·24, W = 48·2.

(March, $\bar{Z} = +0''.04$)

Year.	t	z	Σw^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	Logarithms z	w	t	Numbers zw	w	fw
1	19·1	−0″06	6·3	10	0·10	0·1	1·954	0·806	8·778	1·148	1·281	− 0·85	14·1	268
2	25·8	+1·06	3·5	11	1·02	11·4	2·041	1·173	0·025	0·868	1·412	+ 7·81	7·4	190
3	24·4	−1·70	1·4	5	1·74	15·1	1·301	1·218	0·230	0·083	1·387	− 2·05	1·0	30
4	17·0	−0·25	18·3	17	0·29	1·4	2·435	1·295	9·398	1·140	1·230	− 3·45	13·8	234
5	Great deviations, the rejection has no influence on the result.													
7	24·5	−0·46	1·7	4	0·50	1·0	1·079	0·431	9·663	0·648	1·389	− 2·05	4·4	109
8	19·0	−0·41	6·0	9	0·45	1·8	1·857	0·892	9·613	0·965	1·279	− 3·78	9·2	175
9	20·8	−0·55	4·1	4	0·59	1·4	1·079	0·740	9·740	0·339	1·318	− 1·20	2·2	46
10	15·5	+1·34	11·2	8	1·30	13·5	1·748	1·410	0·127	0·338	1·190	+ 2·92	2·2	33
											Sum	− 2·55	54·3	1·085
											Logar.	0·407	1·735	3·035

March 20·0, $z = -0''.05$, $W = 54·3$.

(April, Z = +0″·06)

Year.	t	z	Σv^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ	Logarithms z	Logarithms w	t	Numbers zw	Numbers w	Numbers tw
1	17·8	+0″·77	9·8	14	0·71	7·1	2·260	1·228	9·886	1·032	1·250	+ 8·28	10·8	192
2	14·0	+0·90	20·6	22	0·84	14·9	2·665	1·550	9·954	1·115	1·146	+11·72	13·0	182
3	21·5	−0·06	2·4	12	0·12	0·2	2·121	0·415	8·778	1·706	1·332	− 3·05	50·8	1092
4	19·8	−0·52	16·4	9	0·58	3·0	1·857	1·288	9·716	0·569	1·297	− 1·94	3·7	73
5	15·9	−0·41	5·8	8	0·47	1·8	1·748	0·881	9·613	0·867	1·201	− 3·02	7·4	117
7	18·1	−0·51	5·2	6	0·57	2·0	1·477	0·857	9·708	0·620	1·258	− 2·13	4·2	75
9	18·8	+0·52	0·8	4	0·46	0·9	1·079	0·230	9·716	0·849	1·274	+ 3·67	7·0	133
10	16·4	+0·54	6·1	5	0·48	1·3	1·301	0·869	9·732	0·432	1·215	+ 1·46	2·7	44
											Sum	+14·99	99·6	1908
											Logar.	1·176	1·998	3·281

April 19·2, z = +0″·15, W = 99·6.

(May, Z = +0″·18)

Year.	t	s	Σv^2	n	$Z-s$	$n(Z-s)^2$	$n.(n-1)$	X'	s	Logarithms w	t	sw	Numbers w	tw
1	15·4	+0·16	7·9	18	0·01	0·0	2·486	0·898	9·204	1·588	1·188	+ 6·2	38·7	597
2	20·0	+1·17	20·9	17	1·00	17·0	2·435	1·579	0·068	0·856	1·301	+ 8·4	7·2	144
3	19·0	+0·18	16·6	18	0·01	0·0	2·486	1·220	9·255	1·266	1·279	+ 3·3	18·4	351
4	17·8	−0·26	24·8	17	0·43	3·1	2·435	1·446	9·415	0·989	1·250	− 2·5	9·7	173
5	13·3	−0·13	19·7	11	0·30	0·9	2·041	1·314	9·114	0·727	1.124	− 0·7	5·3	71
7	14·1	−0·74	15·2	11	0·91	9·1	2·041	1·386	9·869	0·655	1·149	− 3·3	4·5	64
8	20·0	+0·70	3·9	4	0·53	1·1	1·079	0·699	9·845	0·380	1·301	+ 1·7	2·4	48
9	25·1	+0·39	8·8	7	0·22	0·3	1·623	0·959	9·591	0·664	1·400	+ 1·8	4·6	116
10	19·5	+1·12	19·9	13	0·95	11·7	2·193	1·500	0·049	0·693	1·290	+ 5·5	4·9	96
											Sum	+20·4	95·7	1660
											Logar.	1·300	1·981	3·220

May 17·3, s = +0·21, W = 95·7.

(June, $Z = +0''.40$)

Year.	t	s	Σw^2	h	$Z-s$	$n(Z-s)^2$	$n.(n-1)$	Σ'	Logarithms s	Logarithms w	t	Numbers sw	Numbers w	Numbers tw
1	17·3	+0″·64	22·8	20	0·24	1·2	2·580	1·380	9·806	1·200	1·238	+10·1	15·8	274
2	21·5	+0·16	13·7	15	0·24	0·9	2·322	1·164	9·204	1·158	1·332	+ 2·3	14·4	309
3	22·0	+0·10	18·5	16	0·30	1·3	2·380	1·297	9·000	1·083	1·342	+ 1·2	12·1	266
5	22·4	+0·26	12·3	14	0·14	0·3	2·260	1·100	9·415	1·160	1·350	+ 3·8	14·4	324
7	22·1	−0·24	7·5	8	0·64	3·3	1·748	1·033	9·380	0·718	1·344	− 1·2	5·2	114
8	10·4	+0·54	25·5	10	0·14	0·2	1·954	1·410	9·732	0·544	1·017	+ 1·9	3·5	36
9	18·2	+0·13	14·9	9	0·27	0·7	1·857	1·193	9·114	0·664	1·260	+ 0·6	4·6	84
10	19·2	+1·80	16·9	8	1·40	15·7	1·748	1·513	0·255	0·235	1·283	+ 3·1	1·7	33
											Sum	+21·8	71·7	1440
											Logar.	1·338	1·853	3·158

June 20·1, $s = +0''.30$, $W = 71·7$

(July, Z = +0″·62)

Year.	t	z	$\Sigma v'$	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	z	Logarithms w	t	Numbers zw	w.	tw
1	16·2	+0′·45	8·6	17	0·17	0·5	2·435	0·959	9·653	1·476	1·210	+13·5	29·9	485
2	20·3	+1·15	14·7	23	0·53	6·5	2·704	1·326	0·061	1·378	1·308	+27·5	23·9	485
3	14·7	+0·42	18·6	15	0·20	0·6	2·322	1·283	9·623	1·039	1·167	+ 4·6	10·9	161
5	18·5	+0·36	34·1	14	0·26	1·0	2·260	1·545	9·556	0·715	1·267	+ 1·9	5·2	96
7	17·0	+0·96	3·5	8	0·34	0·9	1·748	0·644	9·982	1·104	1·230	+12·2	12·7	216
8	20·0	+0·54	18·0	9	0·08	0·06	1·857	1·257	9·732	0·600	1·301	+ 2·2	4·0	80
9	16·0	+0·32	1·6	5	0·30	0·45	1·301	0·312	9·505	0·989	1·204	+ 3·1	9·8	156
10	13·5	+0·80	2·9	2	0·18	0·07	0·301	0·473	9·903	4·828	1·130	+ 0·5	0·7	9
											Sum	+65·5	97·1	1688
											Logar.	1·816	1·987	3·227

July 17·4, z = +0·67, W = 97·1

(August Z = +0″·81)

Year.	t	z	Σv^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	z	Logarithms w	t	zw	Numbers w	tw
1	18·0	+0″·90	16·4	17	0·09	0·1	2·435	1·218	9·954	1·217	1·255	+14·8	16·5	296
2	17·5	+0·72	15·4	22	0·09	0·2	2·665	1·192	9·857	1·473	1·243	+21·4	29·7	520
3	Rejected for 2 or 3 great discrepancies among 7 observ.													
5	4·6	+0·57	3·6	8	0·24	0·5	1·748	0·613	9·756	0·135	0·663	+ 7·8	13·6	63
6	15·2	+0·37	22·7	9	0·44	1·8	1·857	1·389	9·568	0·468	1·182	+ 1·1	2·9	45
7	11·0	−1·00	2·1	4	1·81	13·1	1·079	1·185	0·000	9·894	1·041	− 0·8	0·8	9
8	13·8	+0·15	7·6	6	0·66	2·6	1·477	1·013	9·176	0·464	1·140	+ 0·4	2·9	40
9	13·3	+0·07	7·8	3	0·74	1·7	0·778	0·978	8·845	9·800	1·124	0·0	0·6	8
10	16·7	+0·80	9·8	9	0·01	0·0	1·857	0·991	9·903	0·866	1·223	+ 5·9	7·3	123
											Sum	+50·6	74·3	1104
											Logar.	1·704	1·871	3·043

August 14·9, z = +0·68, W = 74·3.

(September, $Z = +0''.94$)

Year.	t	z	Σw^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	Logarithms z	Logarithms w		Numbers zw	Numbers w	Numbers tw
1	20·0	+1″·59	17·0	11	0·65	4·6	2·041	1·335	0·201	0·706	1·301	+ 8·1	5·1	102
2	16·6	+1·20	17·1	16	0·26	1·1	2·380	1·260	0·079	1·120	1·220	+15·8	13·2	219
3	14·7	+0·76	20·2	11	0·18	0·4	2·041	1·314	9·881	0·727	1·167	+ 4·1	5·3	78
6	Rejected 3 observs., compare Washington Observ. Volume for 1871.													
7	28·0	+0·70	0·6	3	0·24	0·2	0·778	9·903	9·845	0·875	1·447	+ 5·3	7·5	210
8	20·1	+0·74	14·1	7	0·20	0·3	1·623	1·158	9·869	0·465	1·303	+ 2·2	2·9	59
9	22·7	−0·96	15·3	7	1·90	25·3	1·623	1·609	9·982	0·014	1·356	− 1·0	1·0	24
10	11·5	+1·53	14·0	9	0·59	3·1	1·857	1·233	0·185	0·624	1·061	+ 6·4	4·2	48
											Sum	+40·9	39·2	740
											Logar.	1·612	1·593	2·869

September 18·9, $s = +1''.04$, $W = 39''.2$

(October, Z = +0″·95)

Year.	t	z	Σv^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ	z	Logarithms w	t	zw	Numbers w	tw
1	15·2	+0″·93	5·9	11	0·02	0·0	2·041	0·771	9·968	1·270	1·182	+17·3	18·6	283
2	18·0	+0·91	9·0	14	0·04	0·2	2·260	0·964	9·959	1·296	1·255	+18·0	19·8	356
3	18·2	+1·44	24·9	14	0·49	3·4	2·260	1·452	0·161	0·808	1·260	+ 9·3	6·4	117
7	19·5	+1·00	5·1	2	0·05	0·0	0·301	0·708	0·000	9·593	1·290	+ 0·4	0·4	8
8	18·2	+1·16	6·7	8	0·21	0·4	1·748	0·851	0·065	0·897	1·260	+ 9·2	7·9	144
10	17·0	+0·50	17·5	13	0·45	2·6	2·193	1·303	0·699	0·890	1·230	+ 3·9	7·8	132
											Sum	+57·1	60·9	1040
											Logar.	1·757	1·785	3·017

October 17·1, z = +0·94, W = 60·9

(November, $Z = +0''.87$)

Year.	t	z	Σv^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	Logarithms s	w	t	Numbers sw	w	tw
1	11·3	+1·13	14·5	13	0·26	0·9	2·193	1·188	0·053	1·005	1·053	+11·4	10·1	114
2	15·8	+0·80	25·3	17	0·07	0·1	2·435	1·408	9·903	1·027	1·199	+ 8·5	10·6	168
3	13·5	+0·66	25·3	12	0·23	0·6	2·121	1·413	9·820	0·708	1·130	+ 3·4	5·1	69
7	14·7	+0·29	4·3	7	0·58	2·4	1·623	0·826	9·462	0·797	1·167	+ 1·8	6·3	92
8	8·9	+1·30	9·4	8	0·43	1·5	1·748	1·037	0·114	0·711	0·949	+ 6·7	5·1	46
9	17·3	0·00	15·2	9	0·87	6 8	1·857	1·342	$-\infty$	0·515	1·238	0·0	3·3	57
10	13·8	+0·62	10·2	9	0·25	0·6	1·857	1·033	9·792	0·824	1·140	+ 4·1	6·7	92
											Sum	+35·9	47·2	638
											Logar.	1·555	1·674	2·805

November 13·5, $z = +8·76$, $W = 47·2$

(December, Z = +0″·67).

Year.	t	z	Σw^2	n	$Z-z$	$n(Z-z)^2$	$n.(n-1)$	Σ'	Logarithms z	Logarithms w	Logarithms t	Numbers zw	Numbers w	Numbers tw
1	10·7	+0″·20	4·2	12	0·47	2·5	2·121	0·826	9·301	1·295	1·029	+ 3·9	19·7	211
2	14·7	+1·33	15·8	12	0·66	5·2	2·121	1·322	0·124	0·799	1·167	+ 8·4	6·3	92
3	13·9	+0·94	6·9	10	0·27	0·7	1·954	0·881	9·973	1·073	1·143	+11·0	11·8	164
6*	11·0	−0·70	9·6	2	1·37	3·8	0·301	1·127	9·845	9·174	1·041	− 0·1	0·1	2
7	8·3	−1·85	41·9	6	2·52	38·1	1·477	1·903	0·267	9·574	0·919	− 0·7	0·4	3
8	19·0	+3·03	61·3	7	2·36	39·0	1·623	2·001	0·481	9·622	1·279	+ 1·3	0·4	8
9	12·4	0·00	23·5	9	0·67	4·0	1·857	1·439	− ∞	0·418	1·093	0·0	2·6	32
10	18·2	+1·05	15·4	6	0·38	0·9	1·477	1·212	0·021	0·265	1·260	+ 1·9	1·8	34
											Sum	+25·7	43·1	546
											Logar.	1·410	1·634	2·737

December 12·7, z = +0·60, W = 43·1

* N.B. Rejected only one observation in Dec. 1871, Dec. 27, C.—O. = −21″·4 : great deviations in 1872 and 1873.

Washington, 29 Sept. 1880.

Addition to a Paper on the Method of Least Squares (vol. ɔ pp. 600–614). By J. W. L. Glaisher, M.A., F.R.S.

§ 27. The expressions given in § 14 for ϵ_x^2, ϵ_y^2, &c., squares of the mean errors of x_o, y_o, &c., admit of being t formed by means of the following theorem :—

If D denote any determinant, and if $D_{m,p}$ denote the d minant formed from it by omitting the m^{th} column and th row, and if $D_{mn,pq}$ denote that formed by omitting the m^{t} n^{th} columns and the p^{th} and q^{th} rows, then

$$DD_{mn,pq} = D_{m,p}\,D_{n,q} - D_{m,q}\,D_{n,p}.$$

Applying the theorem to the numerator of ϵ_x^2, and taking m^{th} and n^{th} columns to be the first and last, and the p^{th} an rows to be the first and last, we obtain the formula

$$\begin{vmatrix} (bb), (bc), \dots (bf) \\ (cb), (cc), \dots (cf) \\ \dots\dots\dots \\ (fb), (fc), \dots (ff) \end{vmatrix} \begin{vmatrix} (aa), (ab), \dots (af), (an) \\ (ba), (bb), \dots (bf), (bn) \\ \dots\dots\dots\dots \\ (fa), (fb), \dots (ff), (fn) \\ (na), (nb), \dots (nf), (nn) \end{vmatrix}$$

$$= \begin{vmatrix} (aa), (ab), \dots (af) \\ (ba), (bb), \dots (bf) \\ \dots\dots\dots \\ (fa), (fb), \dots (ff) \end{vmatrix} \begin{vmatrix} (bb), \dots (bf), (bn) \\ \dots\dots\dots \\ (fb), \dots (ff), (fn) \\ (nb), \dots (nf), (nn) \end{vmatrix}$$

$$- \begin{vmatrix} (ba), (bb), \dots (bf) \\ \dots\dots\dots \\ (fa), (fb), \dots (ff) \\ (na), (nb), \dots (nf) \end{vmatrix} \begin{vmatrix} (ab), \dots (af), (an) \\ (bb), \dots (bf), (bn) \\ \dots\dots\dots \\ (fb), \dots (ff), (fn) \end{vmatrix},$$

in which the product of two determinants of $\mu-1$ and rows respectively is equated to the difference of the produ pairs of determinants each of which contains μ rows.

The value of $(m-\mu)\epsilon_x^2$ may therefore be written

$$\frac{\begin{vmatrix} (bb), \dots (bf), (bn) \\ \dots\dots\dots \\ (fb), \dots (ff), (fn) \\ (nb), \dots (nf), (nn) \end{vmatrix} \begin{vmatrix} (ab), \dots (af), (an) \\ (bb), \dots (bf), (bn) \\ \dots\dots\dots \\ (fb), \dots (ff), (fn) \end{vmatrix}^2}{\begin{vmatrix} (aa), (ab), \dots (af) \\ (ba), (bb), \dots (bf) \\ \dots\dots\dots \\ (fa), (fb), \dots (ff) \end{vmatrix} \begin{vmatrix} (aa), (ab), \dots (af) \\ (ba), (bb), \dots (bf) \\ \dots\dots\dots \\ (fa), (fb), \dots (ff) \end{vmatrix}^2},$$

the second term of which is the value of x_o^2.

The first term may be written

$$\frac{\begin{vmatrix} (an), & (ab), \ldots & (af) \\ (bn), & (bb), \ldots & (bf) \\ \cdot & \cdot \cdot \cdot & \cdot \\ (fn), & (fb), \ldots & (ff) \end{vmatrix} \cdot \begin{vmatrix} (nn), & (nb), \ldots & (nf) \\ (bn), & (bb), \ldots & (bf) \\ \cdot & \cdot \cdot \cdot & \cdot \\ (fn), & (fb), \ldots & (ff) \end{vmatrix}}{\begin{vmatrix} (aa), & (ab), \ldots & (af) \\ (ba), & (bb), \ldots & (bf) \\ \cdot & \cdot \cdot \cdot & \cdot \\ (fa), & (fb), \ldots & (ff) \end{vmatrix} \cdot \begin{vmatrix} (na), & (nb), \ldots & (nf) \\ (ba), & (bb), \ldots & (bf) \\ \cdot & \cdot \cdot \cdot & \cdot \\ (fa), & (fb), \ldots & (ff) \end{vmatrix}},$$

and therefore is equal to $x_0\xi$, where ξ is the value of x given by the equations

$$\left.\begin{aligned} (na)x + (nb)y \ldots + (nf)t &= (nn), \\ (ba)x + (bb)y \ldots + (bf)t &= (bn), \\ \cdot \cdot \cdot

for one of the equations, viz. in the ξ-system (1), replaces th equation

$$(aa)x + (ab)y \ldots + (af)t = (an);$$

in the η-system, it replaces the second equation

$$(ba)x + (bb)y \ldots + (bf)t = (bn),$$

and so on.

These forms of the values of ϵ_x^2, ϵ_y^2, &c., are, so fa know, new.

§ 29. Applying the formulæ of the last section t numerical example in § 20, viz. :—

$$\left.\begin{aligned} x - y + 2z &= 3, \\ 3x + 2y - 5z &= 5, \\ 4x + y + 4z &= 21, \\ -x + 3y + 3z &= 14, \end{aligned}\right\},$$

the normal equations are

$$\left.\begin{aligned} 27x + 6y &= 88, \\ 6x + 15y + z &= 70, \\ y + 54z &= 107, \end{aligned}\right\} \ldots\ldots\ldots$$

ξ is the value of x given by

$$\left.\begin{aligned} 88x + 70y + 107z &= 671, \\ 6x + 15y + z &= 70, \\ y + 54z &= 107, \end{aligned}\right\} \ldots\ldots\ldots$$

η is the value of y given by

$$\left.\begin{aligned} 27x + 6y &= 88, \\ 88x + 70y + 107z &= 671 \\ y + 54z &= 107, \end{aligned}\right\} \ldots\ldots\ldots$$

and ζ is the value of z given by

$$\left.\begin{aligned} 27x + 6y &= 88, \\ 6x + 15y + z &= 70, \\ 88x + 70y + 107z &= 671, \end{aligned}\right\} \ldots\ldots\ldots$$

The values of x_0, y_0, z_0 given by (i) are

$$x_0 = \frac{49154}{19899}, \quad y_0 = \frac{2617}{737}, \quad z_0 = \frac{12707}{6633},$$

and solving (ii), (iii), (iv) we find

$$\xi = \frac{60742}{24577}, \quad \eta = \frac{9297}{2617}, \quad \zeta = \frac{24353}{12707},$$

and therefore

$$\epsilon_x^2 = \frac{49154}{19899}\left(\frac{60742}{24577} - \frac{49154}{19899}\right),$$

$$\epsilon_y^2 = \frac{2617}{737}\left(\frac{9297}{2617} - \frac{2617}{737}\right),$$

$$\epsilon_z^2 = \frac{12707}{6633}\left(\frac{24353}{12707} - \frac{12707}{6633}\right),$$

whence

$$\epsilon_x^2 = \frac{1294400}{(19899)^2}, \quad \epsilon_y^2 = \frac{3200}{(737)^2}, \quad \epsilon_z^2 = \frac{65600}{(6633)^2}$$

which agree with the values given by the ordinary process; for

$$(vv) = \frac{1600}{19899},$$

. . . . . .

and, if p_x, p_y, p_z denote the weights of x_o, y_o, z_o,

$$p_x = \frac{19899}{809}, \quad p_y = \frac{737}{54}, \quad p_z = \frac{6633}{122},$$

whence

$$\epsilon_x^2 = \frac{(vv)}{p_x} = \frac{1600}{19899} \times \frac{809}{19899} = \frac{1294400}{(19899)^2},$$

$$\epsilon_y^2 = \frac{(vv)}{p_y} = \frac{1600}{19899} \times \frac{54}{737} = \frac{3200}{(737)^2},$$

$$\epsilon_z^2 = \frac{(vv)}{p_z} = \frac{1600}{19899} \times \frac{123}{6633} = \frac{65600}{(6633)^2}.$$

§ 30. At first sight it would appear that $\epsilon_x=0$, $\epsilon_y=0$, &c., if $x_o=0$, $y_o=0$, &c., respectively: this is not, however, the case, for if, say, $x_o=0$, ξ is infinite. This is evident, since x_o can only vanish if

$$\begin{vmatrix} (an), (ab), \ldots (af) \\ (bn), (bb), \ldots (bf) \\ \ldots\ldots\ldots \\ (fn), (fb) \ldots (ff) \end{vmatrix} = 0;$$

this determinant forms the denominator of ξ, and the numerator of ξ, by § 16, is expressible as a sum of squares, and cannot in general vanish.

When $x_o=0$, the product $x_o\xi$ is to be replaced by its value

$$\begin{vmatrix} (nn), (nb), \ldots (nf) \\ (bn), (bb), \ldots (bf) \\ \ldots\ldots\ldots \\ (fn), (fb), \ldots (ff) \end{vmatrix} + \begin{vmatrix} (aa), (ab), \ldots (af) \\ (ba), (bb), \ldots (bf) \\ \ldots\ldots\ldots \\ (fa), (fb), \ldots (ff) \end{vmatrix}$$

and if $y_o=0$, &c., the products $y_o\eta$, &c., are to be similarly replaced.

§ 31. If $\xi=x_0$, or $\eta=y_0$, &c., then in general ϵ_x, ϵ_y, &c., For since

$$(vv) = -(na)x_0-(nb)y_0- \ldots +(nn),$$

we may regard x_0 as the value of x given by the equation

$$\left.\begin{array}{l}(na)x+(nb)y \ldots +(nf)t = (nn)-(vv),\\ (ba)x+(bb)y \ldots +(bf)t = (bn),\\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\ \cdot\\ (fa)x+(fb)y \ldots +(ff)t = (fn),\end{array}\right\},$$

viz.,

$$x_0 = \frac{1}{\nabla}\begin{vmatrix}(nn)-(vv), & (nb), & \ldots & (nf)\\ (bn), & (bb), & \ldots & (bf)\\ \cdot & \cdot & \cdot & \cdot\\ (fn), & (fb), & \ldots & (ff)\end{vmatrix},$$

and

$$\xi = \frac{1}{\nabla}\begin{vmatrix}(nn), & (nb), & \ldots & (nf)\\ (bn), & (bb), & \ldots & (bf)\\ \cdot & \cdot & \cdot & \cdot\\ (fn), & (fb), & \ldots & (ff)\end{vmatrix},$$

so that if $\xi=x_0$,

$$(vv)\begin{vmatrix}(bb), & (bc), & \ldots & (bf)\\ (cb), & (cc), & \ldots & (cf)\\ \cdot & \cdot & \cdot & \cdot\\ (fb), & (fc), & \ldots & (ff)\end{vmatrix} = 0,$$

the expression on the left-hand side of this equation, wh tiplied by ∇, being in fact the numerator of ϵ_x^2 in § 14. if $\xi=x_0$, then, unless the second factor vanishes, $(vv)=$ therefore $\epsilon_y=0$, $\epsilon_z=0$, &c., and $\eta=y_0$, $\zeta=z_0$, &c. If the factor vanishes, since this determinant is a sum of squa must have

$$\begin{Vmatrix}b_1, & b_2, & b_3 & \ldots & b_m\\ c_1, & c_2, & c_3, & \ldots & c_m\\ \cdot & \cdot & \cdot & \cdot & \cdot\\ f_1, & f_2, & f_3, & \ldots & f_m\end{Vmatrix} = 0,$$

and these conditions imply that the equations of condi not involve the unknowns x, $y \ldots t$ independently, *i.e.* knowns so enter into the m equations of condition that n μ equations, selected from them, affords determinate v x, y, $\ldots$ t.

As mentioned at the end of § 24, if

$$\begin{vmatrix} (bb), (bc), \ldots (bf) \\ (cb), (cc), \ldots (cf) \\ \ldots\ldots\ldots \\ (fb), (fc), \ldots (ff) \end{vmatrix} = 0,$$

then $\nabla = 0$ and therefore also $(vv) = 0$, so that the values of x_0, y_0, &c., p_x, p_y, &c., ϵ_x, ϵ_y, &c., all assume the form $0 \div 0$.

Thus, if any one of the equations

$$\xi - x_0 = 0, \quad \eta - y_0 = 0, \text{ \&c.},$$

is satisfied, in general all the others are satisfied, and ϵ_x, ϵ_y, &c., all vanish. The case of exception is when the equations of condition are not of a form which suffices to determine all the unknowns, and then the whole process of solution becomes illusory.

Note on Hansen's Value of the Semi-diameter of the Moon.
By E. Neison, Esq.

In a Note in the *Monthly Notices* for November 1879 I have stated that, taking 57′ 0″ for the value adopted by Hansen for the constant of parallax, the mean semi-diameter of the Moon, employed by him in his *Tables de la Lune*, is 15′ 33″·47. This result follows from the data given by Hansen in the Introduction to his Tables; though it is not very easy from this source alone to determine the real value of the constants which he has adopted.

In the *Monthly Notices* for June 1880 (page 482), Prof. Adams points out that the mean value of the sine of the equatorial horizontal parallax employed by Hansen in his tables is really 57′ 2″·08. This it seems was pointed out in 1837 by Hansen himsef in the *Astronomische Nachrichten*, vol. xvii. page 299, in a paper which I had not seen until my attention was directed to it by Prof. Adams. The mean semi-diameter of the Moon employed by Hansen in his *Tables de la Lune*, therefore, is 15′ 34′·08, or is 0″·61 greater than stated in my previous Note. All the values given therefore in this Note must be increased by 0″·61, but this correction will not affect the accuracy of the conclusions drawn therein as to the apparent value of the lunar semi-diameter in different instruments. The empirical formula will be: Moon's semi-diameter=

$$15' 33'.98 + 4''.10 + (1 + 0.70 \times \text{aperture in inches}).$$

It may be added that this gives a value for the absolute semi-diameter of Moon equal to

$$15'\ 33''.36,$$

which is in close accord with the value

$$15'\ 33''.35,$$

deduced by myself from the 1500 observations of the Moon made at Greenwich between 1862 and 1877, and with the value

$$15\ 33''.32$$

deduced by Prof. Winnecke of Strasbourg.

I would add that this correction removes a difficulty which has long perplexed me—namely, a sudden alteration in the apparent value of the mean semi-diameter of the Moon at the epoch 1862, the observed values prior and subsequent to that date seeming to differ by rather less than a second of arc. This discordance is referred to in several of my earlier papers, written six or seven years ago, when I was unable to refer to other of Hansen's publications than his Tables.

On the Determination of the Coefficient of the Parallactic Inequality in the expression for the Moon's Longitude. By E. J. Stone, M.A., F.R.S.

In the *Monthly Notices* for 1880, May and June, there appears a paper by Messrs. Campbell and Neison "on the Determination of the Solar Parallax by means of the Parallactic Inequality in the Motion of the Moon."

This paper contains some criticism, to which I do not object, of a paper of mine on the same subject printed in 1867; it contains also some erroneous corrections to the result of that paper to which I must object, and raises some points of interest whic' require examination.

The direct results of Messrs. Campbell and Neison's pap are as follows:—

If we select from the observations of the Moon made Greenwich, with the Transit-Circle, during the years 186' 1876, both inclusive, those which were taken when the M passed within two days of the first and third Quarter, and tl ...ces between the mean results for errors of Han ... Inequality, the coefficient, v

The results are very fairly accordant, and the method is the one which has been previously generally used. This value of the coefficient of the parallactic inequality, 125″·06, would correspond to a value of the solar parallax of about 8″·81.

The coefficient of the parallactic inequality, thus determined, is, however, systematically too small by the amount of the mean excess of the apparent semi-diameters near the Full over those near the two Quarters. The value of the solar parallax 8″·81 is for the same reason too small by about one-fourteenth part of the mean excess of the semi-diameter near the Full, and it can therefore only be regarded as an inferior limiting value of the solar parallax. But although this is clear enough, there are great practical difficulties in investigating the *exact* amount by which the coefficient of the parallactic inequality thus determined should be increased. We know that the required increase cannot exceed 2″, and it most probably does not amount to quite half that quantity; this fixes, with the value of the coefficient 125″·06, which is itself of course liable to error, a superior limit to the value of the solar parallax of about 8″·96, and an inferior one of about 8″·81.

Messrs. Campbell and Neison have attempted to correct for the effects of irradiation as follows:—

They assume semi-diameter near the Quarters

= semidiameter near Full Moon − 0·25I for the winter observations,

= semidiameter near Full Moon − 0·75I for the summer observations,

and determine the quantity I by equating the values of the coefficient of the parallactic inequality which is derived from the discussion of the summer and winter observations. The result thus obtained for the coefficient of the parallactic inequality, 125″·66, is so far satisfactory that it gives an increase of about 0″·6 for the mean excess of semi-diameter near the Full over that of the Quarters, a quantity which from general considerations might be assumed as an approximation to the truth.

The value 125″·66 of the coefficient of the parallactic inequality corresponds to a value of about 8″·85 of the solar parallax. But the weak point in this argument is the assumption of the coefficients 0·25 and 0·75 of the unknown quantity I in the winter and summer equations of condition. It is not stated *how it is found* that these coefficients are approximately correct. Unless it can be shown that there are no admissible variations in the ratio of these assumptions sufficiently important to change the deduced value of the coefficient of the parallactic inequality, the assumptions themselves are not legitimate; and, although the step may be disguised, the investigation is as little satisfactory as the direct assumption, from general considerations, of the irradiational enlargement.

The following table of the values of I for the different years is given by Messrs. Campbell and Neison:—

Year.	Value I.
1876–1875	I = + 11″·92
1875–1874	= − 2·76
1874–1873	= − 7·96
1873–1872	= − 3·61
1872–1871	= − 5·22
1871–1870	= − 4·75
1870–1869	= − 0·56
1869–1868	= + 4·35
1868–1867	= + 5·06
1867–1866	= − 4·03
1866–1865	= − 1·88
1865–1864	= − 1·68
1864–1863	= − 0·74
1863–1862	= − 2·22

Upon these discordant results the authors remark as follows:—"It is obvious that any results based on such wild values must be fallacious. It shows the misleading character of values obtained from the means of large numbers of observations without examining the separate results to see if they are really accordant. The result of the preceding investigation might be regarded as discouraging, and as showing that it would not be practicable to determine in this manner the effect of the variation in the amount of irradiation at the limb of the Moon. If this were really the case, it would be most unsatisfactory, and throw grave doubt on the possibility of determining the true value of the parallactic inequality from observations of the transit of the lunar limb. For the only permissible method of ascertaining the amount of this variation in the irradiation is by this process of comparing the summer with the winter observations. If this prove to be impracticable, it involves as a necessary consequence the failure of the whole method of determining the solar parallax from these observations.

"Further consideration shows that these results are too discordant for this view to be adopted. These extreme discordances can only arise from systematic errors due to imperfections in the tables, or method of reducing the observations. These discordances must be due to the failure of the assumption which we have adopted, in common with Mr. Stone and Prof. Newcomb, and taken as the basis of our work—the assumption that in the mean of a number of observations all errors except those of the parallactic inequality can be neglected and be trusted to destroy one another."

I may mention that these remarks are not applicable to my work. I, at least, did not assume that the errors of any existing lunar tables would mutually destroy each other's effects in obser-

vations spread over half a year. But, waiving this point, which is not important, I pass to the consideration of what would appear to be the chief result of Messrs. Campbell and Neison's paper.

To avoid the unsatisfactory results found by the method already explained, the authors introduce into the equations of condition "twelve unknown quantities, or rather variable corrections," but the *same fundamental assumptions are made with respect to the irradiation.*

The value of the coefficient of the parallactic inequality thus found is 125″·64, a result almost identical with that obtained in the second case without the introduction of the additional corrections. But the importance of the additional corrections appears to me much over-estimated, and to test this point I have calculated, from the data of the paper, the values of I for the different years, and have obtained the following residual errors:—

	Residuals from Incomplete Solution.	Residuals from Complete Solution.
1876-1875	+12″·60	+9″·43
1875-1874	− 2·08	−2·34
1874-1873	− 7·28	−5·94
1873-1872	− 2·93	−1·16
1872-1871	− 4·54	−0·88
1871-1870	− 4·07	−1·11
1870-1869	+ 0·12	+5·28
1869-1868	+ 5·03	+8·68
1868-1867	+ 5·74	+6·85
1867-1866	− 3·35	−2·29
1866-1865	− 1·20	−1·66
1865-1864	− 1·00	−2·50
1864-1863	− 0·06	−3·33
1863-1862	− 0·54	−0·14

It appears to me that the residuals from the general investigation are hardly less unsatisfactory than those previously obtained, and that the introduction of these "twelve unknown quantities, or variable coefficients," and the use of methods with some pretensions to refinement and precision, have not led to a much more satisfactory result than the simpler investigation which has been so severely condemned.

The probable error attached by the authors to the result 125″·64 is only +0″·09, and the value of the solar parallax is 8″·848±0·007.

It is inconceivable to me how such probable errors have been obtained.

To show that I have not objected to the method adopted by Messrs. Campbell and Neison for the determination of the irra-

diation without some grounds, I give the following numerical illustration of my objection.

Messrs. Campbell and Neison find I from the equations

$$(1)\quad mI + dP = -2''.00$$
$$(2)\quad nI + dP = -1''.40,$$

which give

$$I = -\frac{0''.60}{m-n}$$

and

$$dP = -1''.70 + \frac{m+n}{m-n}\,0''.30,$$

or

$$P = 125''.06 + \frac{\frac{m}{n}+1}{\frac{m}{n}-1}\,0''.30.$$

The quantity $125''.06$ is derived from the general investigation and not strictly from (1) and (2) or from the adopted value minus $1''.70$.

Now, with $m = 0.75$, and $n = 0.25$, this gives

$$P = 125''.66.$$

But supposing we take $m = 0.8$, and give to n the values

$$0.2,\ 0.3,\ 0.4,\ 0.5,\ \text{and}\ 0.6$$

respectively, we should then obtain for P the corresponding values

$$125''.56,\ 125''.72,\ 125''.96,\ 126''.36,\ \text{and}\ 127''.16.$$

In fact, we can select a value of the solar parallax and make this result apparently come from a discussion of the observations by determining the ratio $m : n$ from the equation

$$\frac{\frac{m}{n}+1}{\frac{m}{n}-1}\,0''.3 = \delta P,$$

where dP is the correction required by P to give the selected value of the solar parallax.

I want, therefore, an independent proof that $m = 0.75$ and $n = 0.25$ are close approximations to the true values of m and n before I can attach any importance to the correction thus obtained for irradiation.

I now pass to what, if true. would be an important result in Messrs. Campbell and Neison's paper. From the values of the coefficient of the parallactic inequality obtained by the Astronomer Royal in a discussion of the Greenwich Observations 1754 to 1843, Messrs. Campbell and Neison have inferred the existence of an inequality in the Moon's longitude $= 1''{\cdot}20 \sin \{D - 8(y - 1825{\cdot}5)\}$, where D is = mean long. of Moon – mean long. of Sun, and they have shown that such an inequality, if it existed, would change their value of the coefficient of the parallactic inequality from $125''{\cdot}64$ to $124''{\cdot}64$, and value of the solar parallax from $8''{\cdot}85$ to $8''{\cdot}78$.

No attempt is made to *prove* the *necessary existence* of such an inequality; and although, of course, its existence can be proved by its satisfying a sufficient number of the results of observation, yet if it rests simply upon this basis it is absolutely necessary that the results, derived from the assumption of its truth, should agree, not only with the old imperfect observations from which it was derived, but also with the modern observations which have been made with greatly improved instrumental means.

That it does not satisfy the results of observation has not altogether escaped the notice of the authors, for on page 405 we find:—"The computed value of the correction for the group 1843–1851 is $122''{\cdot}75$, whilst that actually found from the observations is $124''{\cdot}53$, there being a difference of $1''{\cdot}78$. Almost the entire portion of this difference arises from the high values found for the parallactic inequality from the observations made during the years 1846–1851, as those found for the earlier years were much lower."

But although it is thus perceived that their inequality gives nearly a minimum value where it should, according to the observations, have given a maximum value, the authors, apparently, were not prepared to face the fact; and they have attempted to escape the difficulty by reasoning which is certainly curious. They remark:—"It would, in fact, seem as if the apparent semi-diameter of the Moon had increased by $2''$ during these years, either from a change of observers or some other cause. This would have increased the apparent value of the parallactic inequality by $2''{\cdot}6$, and as this increase affects two-thirds of the observations, it would correspond to a fictitious increase of $1''{\cdot}73$, whilst that actually observed was $1''{\cdot}78$." I need hardly point out that this step is equivalent to a tacit assumption of the existence of the forty-five year period inequality, and that discordances between its results and those given by observations arise from errors of observations. Such assumptions are always unsafe, and are particularly unsafe in this case, when the only evidence in favour of the existence of the inequality consists of certain discordances between the results obtained by means of the imperfect instruments in use from 1750 to 1816; and when the discussion of these results is complicated by changes of instruments during the period, and the acknowledged

impossibility of any satisfactory determination of the instrumental semi-diameter upon which so much depends.

In the year 1867, when the question whether the true value of the solar parallax was nearer 8″·58 or 8″·90 was an open one, I selected some 2,000 observations of the Moon made near the Quarters at Greenwich with the Altazimuth and Transit-Circle, to test, for my own satisfaction, the legitimacy of the proposed increase of the solar parallax. My method was the same as that followed by Messrs. Campbell and Neison, in section 1, and the result for the coefficient of the parallactic inequality was 125″·36. I had not the slightest expectation of fixing the coefficient within 0″·1, but I stated then and I believe now that the error cannot amount to more than 0″·4, and is probably much less. The semi-diameter used was that obtained from observations near the Full, and the value of the solar parallax thus deduced, about 8″·84, was therefore a minimum value of the result. This investigation appeared to entirely confirm the necessity of the proposed increase of the solar parallax. The result was briefly indicated in the *Monthly Notices* for Nov. 1867.

Messrs. Campbell and Neison profess their ignorance of the exact observations made use of by me, of the Tables through which I have compared the observations with theory, and of any systematic corrections which I may have applied to the results. Their position would therefore have been logically unassailable had they expressed themselves dissatisfied with the result on account of the impossibility of an examination of its freedom from systematic errors; but, instead of taking this course, they have proceeded to investigate the corrections which my result requires; and after thus reducing it from 125″·36 to 123″·84, they appeal to the difference between this, supposed corrected result, and their own, as tending to strengthen the hypothesis of the real existence of the forty-five year period inequality.

Unfortunately for this argument, the corrections which they have applied to my result are certainly erroneous. The corrections which that result requires, if any, are small, and tend rather to increase than to decrease the resulting value of coefficient.

The observations made use by me in 1867 were:—

(1) The Altazimuth Observations, 1848–1861.

(2) The Altazimuth Observations, 1862–1866.

(3) The Transit-Circle Observations, 1853–1861.

(4) The Transit-Circle Observations, 1862–1865.

The observation in sections (1) and (3) were compared directly with Burckhardt's longitudes. Those in (2) and (4) were compared with Hansen's Tables.

After the discovery, by Professor Adams, of the errors in Burckhardt's expression for the parallax, the whole of the reduced

observations were systematically corrected by the Astronomer Royal for errors of parallax and semi-diameter. It is these corrected results which were used by me. The semi-diameters were carefully discussed and separate determinations made for the two instruments, and the corrections required to the coefficient of the parallactic inequality on account of the errors in the coefficient of Burckhardt's variation, annual equation and *term* sin 3D were determined in mean results and applied.

As the results for the Altazimuth instrument extend now over more than thirty years, and are valuable, I give the separate results, extracted from my old papers, for the years 1848 to 1866, and have extended them to 1878 on the same principles. I profess here no attempt to secure greater accuracy than $0''\cdot2$.

The results 1848–1861 are derived from the errors of Burckhardt's longitudes after the observations have been reduced with Adams's parallax and a mean semi-diameter $= 15'\ 34''\cdot52$.

From a direct comparison between the values of the semi-diameters adopted by Adams and Hansen we have

$$\text{Adams' = Hansen's } +0''\cdot52.$$

The corrections to Hansen's semi-diameter now used at Greenwich are

$+0''\cdot49$ for Azimuth Observations,

$+1''\cdot01$ for Altitude Observations.

My value agrees very closely indeed with that derived from the Azimuth Observations and, although it is slightly smaller than the mean result from the Azimuth and Altitude Observations, I appear to have investigated the necessary correction; and it is in my opinion doubtful whether it should be changed.

The correction $+1''\cdot56$ has been applied to the results obtained directly from the observations for the errors of the coefficients in Burckhardt's Tables. Almost the whole of this correction for the Altazimuth observations arises from error in coefficient of the term sin 3D.

	Value of P.		Value of P.
1848	$124''\cdot13$	1855	$125''\cdot62$
1849	123·52	1856	125·17
1850	125·70	1857	124·40
1851	126·18	1858	124·08
1852	126·45	1859	125·03
1853	126·98	1860	125·38
1854	127·50	1861	126·17

Mean value from 1848–1856 = $125''\cdot69$; 1833–1861, $125''\cdot59$.

These are derived through Burckhardt's Tables.

From Hansen's Tables with semi-diameter corrected:—

	P.		P.
1862	125″·92	1871	124″·90
1863	124·32	1872	124·80
1864	125·22	1873	125·25
1865	125·12	1874	125·68
1866	125·05	1875	123·36
1867	125·40	1876	125·35
1868	125·04	1877	125·44
1869	124·72	1878	125·81
1870	124·72		

It will be seen that the existence of the inequality proposed by Messrs. Neison and Campbell is not at all confirmed by these results. Instead of requiring a large positive correction about 1850–1855, the value of P is very large; and if it be considered that the results derived through Burckhardt's Tables are not to be trusted for the results of separate years, similar evidence against this assumed inequality is shown by the later results.

The observations about 1870 should give a maximum result, if the inequality really exists; but this is not shown to be the case in the Transit-Circle results collected by Messrs. Campbell and Neison, whilst for the Altazimuth observations we have:—

	Uncorrected.	With Neison's proposed Correction.	
P =	125″·17	124″·33	1862–1867
	124·84	113·66	1868–1872
	125·15	124·31	1873–1878

The greatest discordance between the uncorrected results is 0″·33, whilst this is increased to 0″·67 by the introduction of the correction proposed by Messrs. Campbell and Neison.

It would almost appear that, so far from this forty-five year period inequality satisfying the observations, its effects are nearly reversed, and give a maximum instead of a minimum about 1870, and a minimum instead of a maximum about 1848.

It is clear however to me, that the attempt to introduce this term is a retrograde step, and I have freely criticised the paper because I know how difficult it is to get rid of such results, when once they find their way into our *Notices*.

It appears to me clear that the value of the parallactic inequality does not differ greatly from 125″·23+x, where x is some positive quantity which probably does not differ greatly from 0″·5, but is certainly less than 2″.

In this case the true value of the solar parallax is not

different from 8″·86, and is almost certainly confined within the limits 8″·82 and 8″·97.

The value of these investigations on the Sun's mean distance is chiefly indirect.

There is certainly no error in an assumed value about 8″·88 which is of any direct importance in our present astronomy. That such is the case, is clearly shown by the fact that we are unable by any one method to prove clearly the error in the assumed value, or, in other words, the assumption of a value about 8″·88 leads to no practical errors which can be laid hold of with certainty. If this were not the case, the correction of the assumed value would present no difficulty.

But although this statement is, I believe, correct, yet the indirect effects of these investigations must, if fairly conducted, be of very great importance. I know of no method free from suspicion of liability to systematic errors, and all discordances between the values of the solar parallax deduced from observations must be valuable as affording indications of the existence of systematic errors in the different methods adopted, which indications should ultimately lead to improve methods of observation or reduction.

In my own investigations I have found all sorts of values from 8″·84 to 8″·94, and I am, at present, quite indisposed to accept any one of these values to the exclusion of a general mean.

The Discovery of κ Pegasi as a close Double Star.

By S. W. Burnham, Esq.

I have recently found that *κ Pegasi* is a very close double star. There is not more than half a magnitude difference in the components. Unfortunately, during the last three months, nights when stars of this class could be properly measured have been rare, and I have only been able to secure four observations, as

A and B.

141°·0	0″·32	1880·613
133·7	0·30	·627
139·8	0·23	·725
135·2	0·22	·766
137·9	0·27	1880·68

These distances are too large; I am confident the true distance will not exceed 0″·2. It is a fine test for the real working

power of a telescope. Only those of the best definition will show it well.

As a wide pair, this star has been known since the time of Sir William Herschel (H N. 43), and is Σ 2804 of the Dorpat Catalogue. My measures of the distant star were generally made when the conditions were too unfavourable to observe the close pair.

A, B and C.

304°·0	11″·55	1880·553
302·3	·90	·613
301·7	·85	·627
304·6	·75	·695

There is some change in angle and distance, due to the proper motion of the large star, as will appear from the following mean results:—

Struve	308°·5	11″·01	1831·56
Dembowski	303·9	11·60	1864·84
Burnham	303·1	11·76	1880·60

The physical relation of the close pair is manifest from the common proper motion of the components, as otherwise it would have been a very easy pair at the time of Struve's measures of the distant star, and certainly would have been detected by him.

Chicago,
1880, Oct. 13.

Observations of Ceraski's Variable Star in Cepheus.

By George Knott, Esq.

Having obtained observations of two recent minima of Ceraski's interesting short-period variable star in *Cepheus*, B.D. +81° 25, I beg to present them to the Society.

Warned by Lord Lindsay's Dun Echt Circular No. 10, which reached me on the morning of October 23, I was prepared to watch for the minimum predicted for the same night, which was fortunately fairly clear, with the exception of a cloudy interval which prevented all observation between $9^h\ 5^m$ and $10^h\ 25^m$. The night of October 28, the date of the next observable minimum, was hopelessly cloudy, and no observations of any kind were possible; but November 2 was fine, and the minimum was observed under favourable circumstances.

The magnitude of the variable at the several times was determined partly by gauging (by the method of limiting apertures),

and partly by comparing it with neighbouring stars, the magnitudes of which are gauged. The magnitude scale is practically that of the *Durchmusterung*, the assumed light-ratio being 2·512. The dates of observation are in Greenwich Mean Time :—

Minimum of October 23.

Time of Observation.	Star's observed Magnitude.	Time of Observation.	Star's observed Magnitude.
h m	m	h m	m
9 5	8·1	12 44	8·8
10 25	9·0	13 7	8·6
10 47	9·05	13 16	8·5
11 7	9·1	13 50	8·1
11 39	9·0	14 0	8·0
12 30	8·95		

Minimum of November 2.

Time of Observation.	Star's observed Magnitude.	Time of Observation.	Star's observed Magnitude.
h m	m	h m	m
6 19	7·3	11 0	9·05
6 55	7·3	11 40	9·1
7 10	7·3	12 10	9·05
7 52	7·4	12 20	8·8
8 0	7·4	12 30	8·6
8 18	8·1	12 47	8·4
8 43	8·4	13 0	8·1
9 35	8·9	13 10	8·0
9 45	9·0	13 55	7·4
10 30	9·1	14 5	7·3

Projecting the observations on cross-ruled paper, and reading off from the light curve, I inferred October 23 $11^h\ 10^m$ as the date of the first minimum. It is quite possible, however, that the slight changes about minimum may be more apparent than real, and that the epoch of mean phase should be taken about 17^m later. The minimum of November 2 appears to have occurred at about $11^h\ 0^m$.

When at its full brightness, the colour of the Variable is white, or bluish white, very similar to that of its neighbour B.D.+81° 30. At the minimum it is ruddy or ruddy-orange. When observing the star on October 23 I noticed a small bluish companion, of about $11\frac{1}{2}$ mag. P=60° D=10″, and suspected a second, at a slightly greater distance, with angle of position 330°. When the Variable was at minimum on November 2, both these small companion stars were well seen, and judged to be of about the $11\frac{1}{2}$ and 13 magnitudes respectively.

I have gauged the magnitudes of several of the neighbouring

stars in the *Durchmusterung* with the following results, giving for comparison the magnitudes assigned to them in the *Durchmusterung* and in Carrington's Red Hill Catalogue:—

No. in B. D.	Mag. B. D.	Mag. Carrington.	Mag. Knott.
+81° 13	6·5	6·2	6·5
22	9·2	9·5	9·4
26	9·5		10·4
27	8·6	8·7	8·4
30	8·3	8·0	8·1
34	8·7	9·1	8·6

The star No. 26 does not appear to have been observed at Red Hill, and the magnitude estimate in the *Durchmusterung* seems to me to be rather too high.

I may note, in concluding, that the Variable was observed on three occasions at Red Hill in the year 1855, the dates of observation and corresponding magnitudes being December 19, 8·0; 21, 8·0; 30, 9·0. The star was probably near minimum at the last epoch.

Knowles Lodge, Cuckfield,
1880, *November* 11.

On the Annual Parallax of the Star P III 242. By Robert S. Ball, LL.D., F.R.S., Royal Astronomer of Ireland.

In the *Monthly Notices* vol. xx. p. 8 is a paper by O. Struve "On a Star which would be suitable for a Parallax Series." The star in question is P III 242, and its position for 1879·0 is $\alpha = 3^h\ 59^m\ 30^s$ and $\delta = +37°\ 45'·3$. Argelander in his Catalogue of 560 stars had pointed out that P III 242 probably formed a wide binary system with the next following star of his Catalogue 50 *Persei*, on account of the equality in direction and quantity of their proper motions.

Struve remarks also that P III 242 is a double star of Herschel's first class (No. 531 of O. Struve's Catalogue), the components being of 6–7th and 8–9th magnitudes respectively, and 3″ or 4″ distant. The physical connection of the two components of P III 242 appears to be established by their equal proper motions. A fourth star, Arg. zone +37°, No. 877 (Mag. 7–8) is south preceding P III 242 at a distance of nearly 4′. This star does not belong to the system formed by P III 242 and 50 *Persei*, for the changes in its position with respect to P III 242 correspond exactly to the proper motion of the latter. "This star, therefore," says Struve in conclusion, "would be a very qualified object of comparison for determining the relative parallax of P III 242, for which a considerable amount is indicated

by the proper motion and by the probable physical connection with 50 *Persei* at 15′ distance."

So far as I know, no measures have hitherto been published with the view of testing whether Struve's surmise as to the existence of a parallax for P III 242 could be substantiated. I therefore commenced a series of observations in January 1879 of the distance and position of the comparison star which Struve suggested from the larger star of the pair P III 242. The instrument employed was the South Equatoreal at Dunsink Observatory, with the Pistor and Martin filar micrometer. The methods of observation and of reduction are the same as those employed by Dr. Brunnow and myself in previous investigations. See Parts I., II., III. of the "Dunsink Observations."

In an important feature, however, the observations now to be discussed are very different from those which had previously been made with our micrometer. The distance in this case was no less than 237″±, which is greatly in excess of the distances measured in the regular series of parallax observations contained in our previous publications. Had there been any suitable comparison star nearer to P III 242 I should certainly have preferred it, for the distance 237″ is too great to be measured by our micrometer with the accuracy which can be attained when the distance is only about one or two minutes. From this cause the results of the observations are not so satisfactory as I would wish, though they are quite sufficient to show that P III 242 has no large parallax.

The following are the observations which I have made of the distance and position, corrected for refraction and reduced to the mean places of the stars at the epoch 1879·0:—

Dist. and Pos. of A.Z. +37°, No. 877 from P III 242.

Date 1879.	Distance.	Position.	
	″	°	″
Jan. 11	237·558	207	17·35
Feb. 7	237·529	207	23·47
19	237·074	207	13·16
22	237·174	207	16·04
23	237·695	207	10·25
25	237·348	207	16·96
Mar. 1	237·220	207	9·08
19	237·605	207	15·99
Apr. 4	237·422	207	9·27
6		207	14·00
10	237·495	207	15·66
Aug. 14	237·268	207	19·18
22	236·973	207	8·47
23	237·177	207	12·83

Date.	Distance.	Position.	
1879.			
Sept. 17	237″·242	207°	16″·28
Oct. 4	237·151	207	20·24
5	237·203	207	17·28
17	237·497	207	16·14
25	237·315	207	17·12
25	237 049	207	22·83
Nov. 1	236·965	207	8·40
2	237·305	207	17·66
8	237·041	207	19·72
8	237·423	207	21·57
11	237·588	207	12·86
Dec. 3	237·647	206	59·53
5	237·186	207	5·91
17	237·587	207	25·42
18	237·215	207	29·27
24	237·130	207	22·88
1880.			
Jan. 9	236·715	207	20·66

According to O. Struve (loc. cit.), the annual proper mo in right ascension is $+0^s\cdot0167$, and in declination $-0''\cdot152$. hence appears that the arc moved over in one year by P III is $0''\cdot2497$, while the position angle of the star in the positio will occupy next year measured from the present positio $127°\cdot5$. The correction to be applied to the observed distanc order to reduce the observed distance to that between the pl at the epoch is $+0''\cdot04407$ per annum, while the correspond correction to the observed position angle is $-3'\cdot565$, or in $-0''\cdot2457$.

The adopted mean distance at the epoch is $237''\cdot320$, and adopted mean position is $207° 13'\cdot86$.

From the usual formulæ it is found that when $\odot$ is the S longitude, R the Sun's radius vector, and π the annual para of P III 242, the correction to be applied to the observed dist to clear it from the effects of parallax is (the figures in brac denoting logarithms)

$$-[9\cdot82787]\pi R \cos(\odot - 174^\circ\ 56'\ 0'')$$

while the corresponding correction of the observed pos angle is

$$-[9\cdot90007]\pi R \cos(\odot - 142^\circ\ 8'\ 29'').$$

Assuming that $-x$ is the correction to be applied to

assumed mean distance, while x' is the correction to the assumed value of the proper motion in distance, and κ is a possible difference in the coefficients of aberration of the two stars, then we have from the observations of the distance the following equations of condition:—

P III 242.

Equations of Condition, from Distance.

					Residuals.	Weight.
x	$+ \cdot0274\,x'$	$+ \cdot2877\,\pi$	$- \cdot6038\,\kappa$	$+ \cdot239 = 0$	$+ \cdot021$	
	$+ \cdot1013$	$+ \cdot5308$	$- \cdot4011$	$+ \cdot214 = 0$	$+ \cdot052$	
	$+ \cdot1342$	$+ \cdot6034$	$- \cdot2791$	$- \cdot240 = 0$	$- \cdot374$	$\frac{1}{2}$
	$+ \cdot1424$	$+ \cdot6175$	$- \cdot2466$	$- \cdot140 = 0$	$- \cdot268$	
	$+ \cdot1451$	$+ \cdot6218$	$- \cdot2357$	$+ \cdot382 = 0$	$+ \cdot257$	
	$+ \cdot1506$	$+ \cdot6299$	$- \cdot2134$	$+ \cdot035 = 0$	$- \cdot086$	
	$+ \cdot1615$	$+ \cdot6438$	$- \cdot1684$	$- \cdot093 = 0$	$- \cdot209$	
	$+ \cdot2108$	$+ \cdot6672$	$+ \cdot0402$	$+ \cdot294 = 0$	$+ \cdot229$	
	$+ \cdot2546$	$+ \cdot6338$	$+ \cdot2212$	$+ \cdot113 = 0$	$+ \cdot081$	
	$+ \cdot2711$	$+ \cdot6088$	$+ \cdot2851$	$+ \cdot187 = 0$	$+ \cdot172$	
	$+ \cdot6160$	$- \cdot5645$	$+ \cdot3734$	$- \cdot025 = 0$	$+ \cdot057$	$\frac{1}{2}$
	$+ \cdot6379$	$- \cdot6091$	$+ \cdot2953$	$- \cdot319 = 0$	$- \cdot241$	$\frac{1}{2}$
	$+ \cdot6407$	$- \cdot6139$	$+ \cdot2851$	$- \cdot115 = 0$	$- \cdot038$	
	$+ \cdot7091$	$- \cdot6739$	$+ \cdot0113$	$- \cdot047 = 0$	$+ \cdot011$	
	$+ \cdot7557$	$- \cdot6455$	$- \cdot1825$	$- \cdot136 = 0$	$- \cdot089$	
	$+ \cdot7584$	$- \cdot6421$	$- \cdot1935$	$- \cdot083 = 0$	$- \cdot038$	
	$+ \cdot7913$	$- \cdot5865$	$- \cdot3217$	$+ \cdot212 = 0$	$+ \cdot249$	$\frac{1}{2}$
	$+ \cdot8132$	$- \cdot5352$	$- \cdot4002$	$+ \cdot031 = 0$	$+ \cdot069$	
	$+ \cdot8132$	$- \cdot5352$	$- \cdot4002$	$- \cdot235 = 0$	$- \cdot201$	
	$+ \cdot8323$	$- \cdot4818$	$- \cdot4628$	$- \cdot318 = 0$	$- \cdot286$	
	$+ \cdot8351$	$- \cdot4736$	$- \cdot4712$	$+ \cdot022 = 0$	$+ \cdot053$	
	$+ \cdot8515$	$- \cdot4214$	$- \cdot5187$	$- \cdot241 = 0$	$- \cdot211$	
	$+ \cdot8515$	$- \cdot4214$	$- \cdot5187$	$+ \cdot141 = 0$	$+ \cdot171$	
	$+ \cdot8597$	$- \cdot3935$	$- \cdot5404$	$+ \cdot306 = 0$	$+ \cdot337$	
	$+ \cdot9199$	$- \cdot1611$	$- \cdot6507$	$+ \cdot368 = 0$	$+ \cdot405$	
	$+ \cdot9254$	$- \cdot1384$	$- \cdot6561$	$- \cdot093 = 0$	$- \cdot051$	
	$+ \cdot9583$	$+ \cdot0015$	$- \cdot6709$	$+ \cdot309 = 0$	$+ \cdot357$	
	$+ \cdot9610$	$+ \cdot0133$	$- \cdot6708$	$- \cdot062 = 0$	$- \cdot013$	
	$+ \cdot9774$	$+ \cdot0835$	$- \cdot6655$	$- \cdot147 = 0$	$- \cdot091$	
	$+ 1 \cdot0219$	$+ \cdot2699$	$- \cdot6150$	$- \cdot560 = 0$	$- \cdot480$	

The normal equations deduced by the method of least squares, and making allowance for the weights, are:—

$$
\begin{aligned}
+28{\cdot}000\ x + 17{\cdot}039\ x' - 1{\cdot}1059\ \pi - 8{\cdot}6094\ \kappa + {\cdot}18500 &= 0,\\
+17{\cdot}039\ x + 13{\cdot}505\ x' - 4{\cdot}1275\ \pi - 6{\cdot}6243\ \kappa - {\cdot}58021 &= 0,\\
-\ 1{\cdot}1059\ x - 4{\cdot}1275\ x' + 7{\cdot}1845\ \pi + {\cdot}77499\ \kappa + {\cdot}85472 &= 0,\\
-\ 8{\cdot}6094\ x - 6{\cdot}6243\ x' + 7{\cdot}7499\ \pi + 5{\cdot}3655\ \kappa - {\cdot}029128 &= 0.
\end{aligned}
$$

Solving these, we deduce

$$
\begin{aligned}
x &= -0''{\cdot}1459 \pm 0''{\cdot}08,\\
x' &= +0{\cdot}3009 \pm 0{\cdot}15,\\
\pi &= +0{\cdot}0163 \pm 0{\cdot}09,\\
\kappa &= +0{\cdot}1405 \pm 0{\cdot}12.
\end{aligned}
$$

The sum of the squares of the residuals is 1·2732, from wh the probable error of one observation is $\pm 0''{\cdot}15$. The sum the squares of the absolute terms is 1·4638.

We next proceed to form the equations of condition from observations of the position angle. In a complete series measures four observations of the parallel and four of the posit angle have been made. Owing to the great distance of stars, the measurements of the position angle (estimated in a are not very satisfactory, and on two occasions (Dec. 3 and D 5, 1879) the discrepancies have attained to very undesira dimensions. The residual on Dec. 3 is no less than $-1''{\cdot}3$ but only a weight of ½ attaches to this result, because it v based on but two observations of the parallel and two of position. The notes at the time of observation are, "Snow a severe frost; low and hazy but tolerably steady." On the n night of observation, Dec. 5, the number of observations v complete, and they were fairly accordant; the notes at the ti record, "Good definition; thaw; occasional clouds." The r dual on this occasion is $-0''{\cdot}895$. It will be noticed that th observations occur at dates when the parallax produces but v little effect, the coefficient being +·2536 on the first occasi and +·2801 on the second.

The following are the equations of condition which deduced from the position angle :—

P III 242.

Equations of Condition, from Position Angle.

					Residuals.	Weig
x + ·0287 x'	+ ·6715 π	− ·4089 κ	+ ·234 = 0	+ ·103		
+ ·1028	+ ·7839	− ·0470	+ ·638 = 0	+ ·544		
+ ·1352	+ ·7783	+ ·1186	− ·082 = 0	− ·152		½
+ ·1437	+ ·7714	+ ·1609	+ ·115 = 0	+ ·052		
+ ·1465	+ ·7686	+ ·1752	− ·286 = 0	− ·343		

				Residuals.	Weight.
+ ·1518 x'	+ ·7627 π	+ ·2011 κ	+ ·176 = 0	+ ·120	
+ ·1624	+ ·7480	+ ·2534	− ·370 = 0	− ·417	
+ ·2119	+ ·6367	+ ·4743	+ ·095 = 0	+ ·096	
+ ·2558	+ ·4856	+ ·6308	− ·380 = 0	− ·336	¼
+ ·2613	+ ·4639	+ ·6471	− ·055 = 0	− ·006	⅛
+ ·2723	+ ·4189	+ ·6774	+ ·057 = 0	+ ·116	
+ ·6179	− ·8058	+ ·0053	+ ·215 = 0	+ ·350	
+ ·6398	− ·7980	− ·1014	− ·530 = 0	− ·411	½
+ ·6427	− ·7959	− ·1153	− ·230 = 0	− ·113	
+ ·7106	− ·6747	− ·4268	− ·008 = 0	+ ·049	
+ ·7572	− ·5207	− ·6017	+ ·254 = 0	+ ·265	
+ ·7599	− ·5101	− ·6106	+ ·049 = 0	+ ·057	
+ ·7927	− ·3731	− ·7022	+ ·038 = 0	− ·061	⅛
+ ·8141	− ·2742	− ·7465	+ ·024 = 0	− ·019	
+ ·8143	− ·2735	− ·7468	+ ·419 = 0	+ ·376	
+ ·8335	− ·1805	− ·7748	− ·582 = 0	− ·642	¼
+ ·8364	− ·1663	− ·7780	+ ·056 = 0	− ·006	
+ ·8522	− ·0872	− ·7910	+ ·195 = 0	+ ·120	¼
+ ·8523	− ·0865	− ·7911	+ ·322 = 0	+ ·247	⅛
+ ·8609	− ·0435	− ·7947	− ·281 = 0	− ·362	
+ ·9205	+ ·2536	− ·7531	− 1·216 = 0	− 1·332	½
+ ·9261	+ ·2801	− ·7434	− ·777 = 0	− ·895	
+ ·9598	+ ·4318	− ·6639	+ ·562 = 0	+ ·435	
+ ·9624	+ ·4427	− ·6564	+ ·827 = 0	+ ·699	
+ ·9789	+ ·5091	− ·6044	+ ·381 = 0	+ ·251	
+ 1·0233	+ ·6155	− ·4916	+ ·217 = 0	+ ·090	

The normal equations deduced from these by the method of least squares are, after making allowance for the weights:—

$$
\begin{aligned}
+27{\cdot}250\,x &+ 16{\cdot}223\,x' + 4{\cdot}0258\,\pi - 8{\cdot}1716\,\kappa + 1{\cdot}0844 &&= 0,\\
+16{\cdot}223\,x &+ 12{\cdot}708 - {\cdot}1377 - 8{\cdot}3986 + {\cdot}77684 &&= 0,\\
+\ 4{\cdot}0258\,x &- {\cdot}1377 + 8{\cdot}5200 + 1{\cdot}6929 + {\cdot}85405 &&= 0,\\
-\ 8{\cdot}1716\,x &- 8{\cdot}3986 + 1{\cdot}6929 + 8{\cdot}6527 - {\cdot}78159 &&= 0.
\end{aligned}
$$

From which we obtain—

$$
\begin{aligned}
x &= +0''{\cdot}0185 \pm 0''{\cdot}14,\\
x' &= +0{\cdot}0076 \pm 0{\cdot}27,\\
\pi &= -0{\cdot}1371 \pm 0{\cdot}11,\\
\kappa &= +0{\cdot}1420 \pm 0{\cdot}18.
\end{aligned}
$$

The sum of the squares of the absolute terms of the equations is +4·3105, and the sum of the squares of the residuals is +4·1077; from whence the probable error in arc of one complete observation of the position angle is

$$\pm 0''.263.$$

If we combine the two values of the parallax, making due allowance for the weights, we obtain as the final result of this series of observations

$$\pi = -0''.045 \pm 0''.070.$$

If this result were to be strictly interpreted, it would mean that the comparison star was actually the less distant of the two. Observing, however, that the probable error is greater than the parallax itself, it would seem unsafe to draw any conclusion from these observations, except that the difference between the parallaxes of P III 242 and +37° 877 is too small to be measured with accuracy.

A Determination of the Diameter of Mars at the Mean Distance of the Earth from the Sun. By A. M. W. Downing.

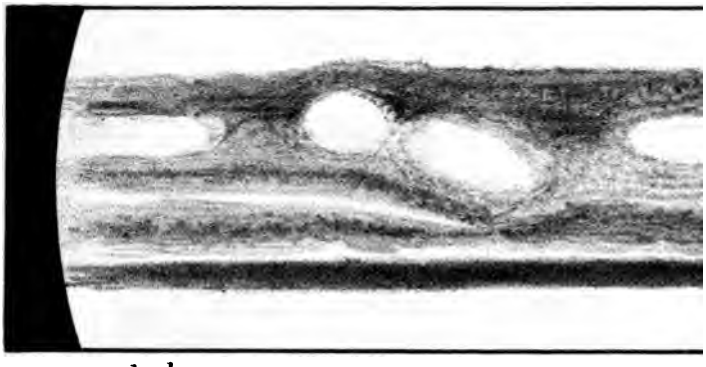
Recent investigations have shown conclusively that Le Verrier's value of the diameter of *Mars*, which has been adopted in the English *Nautical Almanac* since 1866, is considerably too large.

Thus the diameter according to

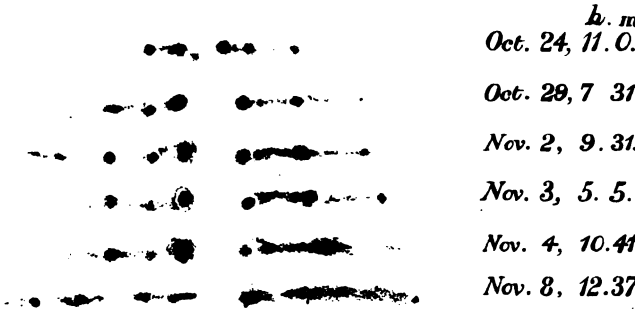
Le Verrier is	11''·10	(*Annales de l'Observatoire*, tome vi.)
Hartwig	„ 9·352	(*Publ. Ast. Ges.*, No. xv.)
Young	„ 10·068	(*Am. Journal*, March 1880)
Pritchett	„ 9·486	(*A. N.* No. 2309).

Under these circumstances it is desirable that the value of the diameter of *Mars*, found from the observations made with the Greenwich Transit-Circle, should be computed, and in the present paper I give the results obtained from the investigation. This determination cannot of course compare in accuracy with values derived from heliometer or other double-image measures, but is rather intended to supply the proper value of the diameter to be used when from accident of weather, or defect of illumination, only one limb of the planet can be observed at Greenwich. It must, however, be remembered that we have more than 500 measures of the diameter made with the same instrument, since its erection in 1851.

The Great Red Spot.



Oct, 18th. 11h 20m. Portion of Gt Equatorial belt on W. limb. showing two white spots.



Group of Dark Spots in N. Hemisphere.

Pottiswoode & Co. Lith. L

From 1851 to 1865, the value of the diameter of *Mars* assumed in the *Nautical Almanac*, and with which the Greenwich observations have been compared, was $8''\cdot87$. Let the true diameter$=8''\cdot87\ (1+y)$, and let x be the constant correction to the assumed value for irradiation, then each observation of vertical diameter of the planet gives an equation of the form

$$x + 8''\cdot87(1+y) = \text{observed diameter.}$$

There are 272 observations of vertical diameter in the years 1851–1865, and the equations of condition being solved by the method of least squares, we have

$$\begin{aligned} 272\ \ x + 3881\cdot2\ \ y &= 983''\cdot13 \\ 3881\cdot2\ x + 59708\cdot42\ y &= 14396\cdot94 \end{aligned}$$

whence

$$\begin{aligned} x &= +2''\cdot399 \pm 0''\cdot261 \\ y &= +0\cdot0852 \pm 0\cdot018, \end{aligned}$$

it being assumed that each observation is a measure of the same physical quantity, *i.e.* neglecting the ellipticity of the disk.

The probable error of a single measure of diameter, found from Lüroth's extension of Peters' formula, viz.,

$$\epsilon = 0\cdot8453\ \frac{\Sigma e}{\sqrt{n(n-\mu)}},$$

where Σe is the sum of the residuals without regard to sign, n the number of equations, μ that of the unknown quantities, is

$$\pm 1''\cdot158.$$

From this series of observations therefore we have

$$\text{True diameter} = 8''\cdot87(1+y) = 9''\cdot625 \pm 0''\cdot156. \qquad (1)$$

Since 1866 the value of the diameter adopted is $11''\cdot10$, therefore we have

$$x' + 11''\cdot10(1+y') = \text{observed diameter,}$$

and solving by the method of least squares, the 265 equations derived from the observations of vertical diameter made since 1866, there results: —

$$\begin{aligned} 265\ \ x' + 4488\cdot1\ \ y' &= 78''\cdot88, \\ 4488\cdot1\ x' + 83671\cdot97\ y' &= 412\cdot37, \end{aligned}$$

whence

$$x' = +2''.340 \pm 0''.235,$$
$$y' = -0.1206 \pm 0.013,$$

and

$$\text{True diameter} = 11''.10(1+y') = 9''.761 \pm 0''.147. \qquad (2)$$

Combining (1) and (2), we obtain for the final value of diameter of *Mars* as measured with the Greenwich Transit-Circle—

$$9''.697 \pm 0''.107.$$

If the adopted diameter be 9''·697, x and x' will require slight modifications; let a and b be the corrected values, then

$$a + 9''.697 = 2''.399 + 9''.625, \quad \therefore a = 2''.327$$
$$b + 9.697 = 2.340 + 9.761, \quad \therefore b = 2.404.$$

Combining a and b, we find

$$\text{Constant Correction} = 2''.369 \pm 0''.175.$$

This correction is unusually large; for instance, with the Washington Mural-Circle, the aperture of the object glass of the telescope of which is four inches, the corresponding quantity is found to be 1''·192.

It might be expected from *a priori* considerations that the larger aperture would give a smaller irradiation-constant; there is, however, evidence to show that the value of the correction in question does not appear to follow any law depending on the aperture of the telescope employed.

1880, *November* 11.

The Motions and Varieties of the Jovian Spots.

By W. F. Denning, Esq.

Those who have telescopically observed *Jupiter* during the last few months can hardly fail to have been struck with the very remarkable changes exhibited in the positions and forms of his spots. This remark does not apply, however, to the great red spot which has maintained a constancy of appearance and regularity of motion, in singular contradiction to the white oval patches, alternating with dark spaces along the equator, and to a series of dark spots which have been visible on a belt lying N. of the equatorial zone.

As to the large spot, I always see it with a minute black speck at its interior f. end. This is distinctly visible when either on the

east or west limb; more plainly when in the latter position; so that I infer the great spot is not a cavity, or the black point alluded to, if situated on its sloping side, must disappear as it approached the west limb. The ends of the Great spot taper, as in following sketch (see lithograph). It lies perfectly parallel with the equatorial belt, which shows no perceptible depression, as some observers have intimated, in that portion immediately adjacent to the spot.

On Oct. 18, while watching the transits of Sat. I. and II., I noticed an unusually bright patch projecting out of the great S. belt towards the equator. It was followed by a white oval marking, and as it neared the W. limb I made a sketch as follows (see lithograph).

This object was the brightest and most conspicuous of its class on the planet, and from the date that it was first seen, on Sept. 18 at $11^h\ 50^m$, until Nov. 23, it was carefully reobserved on every possible occasion, and its times of transit across the central meridian cautiously estimated. Relatively to the red spot, it soon manifested a great change of position. It was seen at places considerably in advance of that predicted on the assumption of a rotation period of $9^h\ 55^m\ 34^s$. It passed the central meridian at about $10^h\ 30^m$ on Oct. 18, and on Oct. 20 at $11^h\ 38^m$. The period was approximately $5\frac{1}{2}$ minutes less than the red spot. After Oct. 20th it was closely watched, and by Nov. 21 had been reobserved on thirteen nights. On the latter date it crossed the meridian at $10^h\ 41^m$, and the resulting observed period from a mean of all the observations was $9^h\ 50^m\ 5^s$, which, though depending upon eye-estimations of the transits, cannot be far wrong. If this rapid motion of the spot, from west to east around *Jupiter*, continues, it will complete its circuits of the planet in about 45 days = 108 rotations of the great spot.

Mr. H. Corder at Writtle, near Chelmsford, had, with only a $4\frac{1}{2}$-in. reflector, noticed a similarly rapid movement of the white equatorial spots from his observations in the autumn of 1879 (see *Observatory*, December 1879, pp. 238–9). He inferred an "independent rotation, relatively to the red spot, in about 40 days."

On Oct. 23, at 11^h, I saw several minute, dark objects, looking like a train of Sun-spots, upon a dusky belt to the N. of the great equatorial shading. I saw them again on Oct. 29, 30, 31, Nov. 2, 3, 4, and 8, and they showed remarkable changes. The group was evidently lengthening in the direction of the belt, and the constituent spots varied from day to day. On Oct. 29 the whole group passed the central meridian in less than an hour, whereas on Nov. 8 it occupied double that interval. The spots had further developed to such an extent by Nov. 23 as to range over 120 degrees of longitude, for they were $3^h\ 19^m$ in crossing the meridian of *Jupiter* on that date. I figured the spots each night (see lithograph).

The same swift movement eastwards upon the planet (from east to west observed from the Earth) as that noticed in regard

to the bright patches, at once became evident. The most conspicuous spot of the group was on the central meridian at $7^h\ 31^m$ on Oct. 29, *following* the middle of the great red spot by 23^m. On Nov. 2 the positions were reversed. The smaller spot preceded the red cloud by 55^m. The next night, Nov. 3, the difference had increased to $1^h\ 7^m$, and on Nov. 4 to $1^h\ 23^m$. On Nov. 8 the interval had further increased to nearly $2\frac{1}{4}^h$, and by Nov. 23, to $7^h\ 26^m$.

The relative observed times were:—

	Great spot in S. hem.	Small spot in N. hem.
	h m	h m
Oct. 29	7 8	7 31
Nov. 2	10 26	9 31
3	6 12	5 5
4	12 4	10 41
8	5 21	12 37

In the latter comparison the *preceding* transit of the red spot has been given because the one following occurred when *Jupiter* was very near the horizon and could not be observed. From an analysis of the estimated times these small spots had an observed period slightly exceeding $9^h\ 48^m$, and $7\frac{1}{2}^m$ less than the red spot.

As opposed to these motions I may remark that there is a small elliptical dark spot s.p. the red cloud which has maintained, since its first discovery by Mr. Barnard at Nashville, Tenn., on July 25 last, a permanent position with reference to the red spot, and therefore evidently partakes of the same character.

Ashley Down, Bristol,
1880, *November* 25.

Observations of the Red Spot on Jupiter. By Ankitam Venhata Nursinga Row.

Mr. Nursinga Row writes from Vizagapatam, India, on Jan. 22, 1880:—I have been observing a reddish brown spot crossing *Jupiter's* disk, every second evening from Dec. 27th. It has always an elongated oval form with a sharp point at the western end of its major axis. It is surrounded by an elliptical ring of brilliant white with a yellow tinge on its outer edge, which is brightest when the spot is on the central meridian of the planet.

The longer axis of the elliptic ring is about equal to one-third of the planet's diameter. On the 29th of Dec. I estimated that it crossed the central meridian at $6^h\ 23^m\ 27^s$ P.M., and on the 31st it crossed the central meridian at $7^h\ 58^m\ 15^s$ P.M.

A six-inch achromatic telescope has been employed in making the above observations with an eyepiece giving a power of 150.

Note on a Phenomenon of Jupiter's Satellites. By Capt. W. Noble.

On turning the telescope on to *Jupiter* at $8^h\ 30^m$ L.M.T. on the night of October 18, I noticed, of course, the shadows of the two Satellites which were at that moment crossing the planet's disk. So peculiar an appearance, however, did the preceding one present (that, as I immediately subsequently ascertained, of Satellite II.) that I turned to the *Nautical Almanac* before I could satisfy myself that it was a shadow at all. It was, naturally, smaller than that of Satellite I., which was about a quarter of the way across *Jupiter's* face, and just skirting the south equatorial belt; but what at once arrested my attention was its curiously light colour. In fact, it looked lighter than I have seen Satellite III. itself look while in the act of transiting its primary; and this at first impressed me with the idea that I was really looking at a Satellite, and not at its shadow at all, until reference to the Ephemeris convinced me to the contrary. Briefly then the shadow of Satellite II. appeared like a very small circular spot of a dark brown tint, while that of Satellite I. resembled a tiny disk of ink. The only feasible or conceivable explanation of this curious phenomenon, to me, is this—that while obviously the portions of the planet's disk covered by these shadows suffered a total solar eclipse, the shadow of Satellite II. must have fallen upon a part of it more than ordinarily self-luminous. As a matter of fact it was considerably south of the equator, and in a different zone to that of Satellite I. Power, 154, on my 4·2-inch Ross Equatoreal.

Forest Lodge, Macesfield, Uckfield,
1880, *October* 22.

Note on a Photograph of Jupiter. By A. A. Common, Esq.

On August 9, 1880, a transit of the shadow of the III. Satellite taking place, I took some photographs of the planet, one of which I herewith present to the Society. It was taken at 11 42, G. M. T. and shows *Jupiter* with the bands, the II. and III. Satellites and the shadow of the III. on the disk. The exposure for the planet was about two seconds, for the Satellites twenty seconds. Some photographs have been taken of the red spot, and they are very similar to those taken last year.

Ealing,
November, 1880.

Note on the Nebulæ near Merope. By A. A. Common, Esq.

In the Supplementary Number of the *Notices* of this Society for 1880, there is an extract of a letter from Professor Tempel in which he states that I have evidently made the sketch published in the April Number with a telescope of insufficient power to show the *Merope* Nebula. Perhaps I ought to have stated that the telescope was of three feet *aperture*, but the mention of the fact that the stars on the sketch were pricked off from a photograph taken by the same telescope ought to have shown that it was of ample power. I drew attention to the fact that the patch of light near *Alcyone* was seen but once, but that the smaller patch was always seen as a detached portion. By the aid of a sketch which includes the small stars near, I am enabled to locate the Nebula on the map that Prof. Tempel gives. On this sketch are four small stars nearly at equal distances apart pointing to *Alcyone*, with three others that form with the four an acute triangle with *Merope* for the apex. Now the exact position of this detached Nebula from the sketch I made on February 29, 1880, is this: it commences at the faint star near *Merope* and passes over the three stars already mentioned, the major axis nearly corresponding to the line made by these three faint stars. In my sketch, two faint stars are also shown, and another faint one between the edge of the Nebula and *Merope*, the sky here being quite black—making this a distinct Nebula from that on the other side of *Merope*.

Ealing,
1880, *November* 11.

A New Nebula. By J. Baxendell, Esq.

(*From a letter to W. H. M. Christie, Esq.*)

On the night of September 28, 1880, I observed a large h
faint Nebula which is new to me, and which I do not f
entered in any Catalogue or marked on any map I have in
possession. It is in *Aquarius*, near to and north of the
cluster Messier's No. 2, its southern boundary, so far as I
yet made it out, being only about 7′ distant from the clu
It has an irregular oval form, its longer axis lying in a n
east and west direction. A mean of several estimations giv
length about 1¼°, and its breadth about 52′. It seems
similar in character to the large Nebula near the *Pleiades*,
slightly less bright. I have, however, seen it on several
and have no doubt of its existence.

The Observatory, Birkdale,
1880, *Nov.* 1.

Cometary Observations made at the University Observatory, Oxford. By the Rev. Prof. C. Pritchard.

Hartwig's Comet

Date 1880.	Oxford Mean Time.	$\delta - *$ $\Delta\alpha$	Apparent R.A. of δ.	Oxford Mean Time.	$\delta - *$ $\Delta\delta$	Apparent Declination of δ.	Star of Comparison.
	h m s	′ ″	h m s	h m s	′ ″	° ′ ″	
Oct. 5	9 20 40·9	+ 8·16	15 54 36·88	9 22 23·0	−11 57·7	+25 34 36·8	*a*
11	7 21 56·5	+1 58·48		7 21 56·5	− 1 4·9		
11	7 44 44·4	+1 3·42		7 44 44·4	+ 1 10·1		
14	7 1 29·6	+ 22·93	17 15 26·74	7 1 40·4	− 5 4·1	+18 5 59·2	*b*
18	7 50 16·4	−3 28·16	17 36 11·79	7 50 16·4	+ 3 26·7	+15 37 26·7	*c*
20	6 54 17·8	−2 6·24	17 44 22·89	6 54 17·8	+ 9·0	+14 36 36·5	*d*
23	8 55 4·0	+4 17·18	17 55 25·55	8 57 9·5	+ 7 41·7	+13 15 28·7	*e*
30	6 56 45·3	−3 55·19	18 14 38·51	7 14 59·3	− 4 27·7	+10 49 25·7	*f*
31	6 22 19·9	+ 1·98	18 16 52·89	6 23 32·7	+ 2 49·4	+10 33 20·0	*g*
Nov. 2	6 54 48·5	+ 32·22	18 21 24·26	6 32 59·5	+ 3 5·1	+10 2 6·6	*h*
2	6 54 48·5	+ 22·24	18 21 24·20	6 32 59·5	+ 8·6	+10 2 5·8	*i*
3	6 35 24·9	−1 0·42	18 23 28·77	6 49 17·4	+ 2 3·8	+ 9 47 30·9	*j*
4	6 43 50·1	−2 38·26	18 25 31·45	6 43 50·1	+ 5 43·9	+ 9 33 54·7	*k*

Adopted Mean Places of Comparison Stars, 1880·0.

	R. A.	Declination.	
	h m s	° ′ ″	
a	15 54 27·02	+25 46 31·9	Taylor 8338,
b	17 15 1·63	+18 10 56·1	Rumker 5777 and Taylor 8033,
c	17 39 37·67	+15 33 51·3	Bonn Catalogue + 15° No. 3263,
d	17 46 26·80	+14 36 18·5	Weisse Bessel xvii^h No. 920,
e	17 51 6·01	+13 7 36·9	Weisse Bessel xvii^h No. 1022,
f	18 18 31·23	+10 53 42·5	Weisse Bessel xviii^h No. 392,
g	18 16 48·85	+10 30 21·1	Micrometrically connected with Bonn Cat. + 10° No. 3488,
h	18 20 49·57	+ 9 58 50·7	Weisse Bessel xviii^h No. 452,
i	18 20 59·49	+10 1 46·4	Weisse Bessel xviii^h No. 458,
j	18 24 26·70	+ 9 45 15·8	Bonn Catalogue + 9° No. 3741,
k	18 28 7·23	+ 9 27 55·8	Bonn Catalogue + 9° No. 3771.

Remarks:—Oct. 5. The Comet was rather low when found, and no trace of a tail was visible.

Oct. 14. Bright moonlight and sky foggy: the Comet is now a very inconspicuous object and not seen in a telescope of 4″ aperture.

Oct. 18. The Moon now nearly full and the Comet very faint.

Oct. 30. The Comet, which on this night was very indistinct from fog, was also compared with Weisse Bessel, xviii. No. 383, but this is apparently an error in the right ascension as given by Bessel.

Oct. 31. The sky having clouded suddenly: only one measure of difference of right ascension and of declination could be obtained.

Nov. 4. Sky foggy: observation very uncertain.

Faye's Comet.

Date 1880.	Oxford Mean Time.	δ − α dα.	Geocentric R.A. of δ.	Oxford Mean Time.	δ − ✱ "Δ.	Geocentric Declination of δ.	Correction to Ephemeris. dα.	Correction to Ephemeris. dΔ.	Star of Comparison.
	h m s	m s	h m s	h m s	′ ″	° ′ ″	s	″	
Sept. 6	10 28 13·4	−1 3·60	23 4 7·90	10 28 13·4	− 28·4	+9 17 57·6	−2·17	+ 5·5	*a*
28	10 27 22·6	−1 53·23	22 51 16·44	10 27 22·6	+7 24·0	+5 38 25·9	−2·08	+19·7	*b*
Oct. 2	8 57 48·7	− 54·30	22 49 38·91	9 19 51·7	−1 6·2	+4 52 18·5	−2·04	− 3·9	*c*
5	10 5 50·0	− 45·08	22 48 39·65	10 14 26·5	+5 0·8	+4 16 24·1	−1·42	+ 5·0	*d*
11	10 3 42·6	+ 23·07		10 3 42·6	+4 5·3				
30	8 20 8·5	− 18·89	22 51 34·02	8 12 53·8	+2 27·9	−0 0 22·8	−1·29	+ 6·9	*e*
Nov. 2	7 27 15·4	−1 12·53	22 53 20·15	7 35 29·8	+4 14·6	−0 22 39·8	−2·19	+ 2·1	*f*
3	7 13 30·9	− 32·36	22 54 0·32	7 28 6·7	−2 43·4	−0 29 37·6	−1·95	+ 1·5	*f*
4	7 31 48·0	− 34·38	22 54 43·87	7 44 58·2	−5 40·6	−0 36 27·0	−1·34	+ 1·4	*g*

Adopted Mean Places of Comparison Stars, 1880·0.

	R. A.	Declination.	
	h m s	° ′ ″	
a	23 5 7·21	+9 17 53·1	Micrometrically connected with Rumker 10913,
b	22 53 5·23	+5 30 26·6	Bonn Catalogue +5° No. 5114,
c	22 50 28·89	+4 52 49·4	Bonn Catalogue +4° No. 4927,
d	22 49 20·28	+4 10 48·0	Weisse Bessel xxii^{h} No. 1000,
e	22 51 48·70	+0 1 30·8	Micrometrically connected with W. B. xxii^{h} No. 1192,
f	22 54 28·52	−0 27 28·6	Gr. 12 yr. Cat. No. 2067, and Armagh No. 5066,
g	22 55 14·08	−0 31 20·9	Weisse Bessel xxii^{h} No. 1132.

The observations have been reduced and the comparisons with the Ephemeris have been made with the necessary corrections. This Comet has been so faint during its present apparition that the observations have been made with considerable difficulty. In moonlight it has been impossible to see it with the 12¼-inch Refractor, and from this cause, together with cloudy weather, it has not been observed since the 4th inst. The observations were made, under my direction, by Mr. W. Plummer.

Oxford,
1880, *November* 11.

On the Spectrum of Comet 1880 *d* (*Hartwig's*).
By W. H. M. Christie, Esq.

Cloudy weather prevented any observation of this comet at the Royal Observatory, Greenwich, till October 7, by which time it had considerably diminished in brightness. I observed the spectrum with the single prism spectroscope on the Great Equatoreal on October 7, 11, and 12, and found that it consisted of the three usual cometary bands, the nucleus giving a continuous spectrum.

Measures of the position of the brightest band as compared with the green carbon-band at 5198 (alcohol in a vacuum-tube) on Oct. 7 and 12, and with that at 5165 (blue base of flame) on Oct. 11 and 12, were made with the following results:—

1880	G.M.T. h	m	Comet-band.	Wave-length inferred.		Width of Slit. in.	
Oct. 7	9	0	Red Edge	5198−13 = 5185		0·018	= 35 tenth-metres
	9	0	Red Edge	5198+15 = 5213			
	9	0	Red Edge	5198+ 6 = 5204			
Oct. 11	9	0	Centre	5165−22 = 5143		0·0195	= 38 tenth-metres
	9	20	Centre	5165+36 = 5201			
Oct. 12	7	30	Centre	5198+ 7 = 5205		0·0165	= 32 tenth-metres
	8	0	Centre	5198−44 = 5154			
	8	20	Centre	5198−50 = 5148	}		
		,,	,,	5165− 2 = 5163	}		

MEANS—

		in.	
Oct. 7	5201	0·018	= 35 tenth-metres
Oct 11 and 12	5169	0·018	= 35 tenth-metres

The less refrangible edges of the comet-band and carbon-band were compared on Oct. 7, and the centres of the brightest part of the two bands on Oct. 11 and 12.

These measures leave it uncertain whether the comet-band coincides with the band in the second spectrum of carbon (vacuum-tube) at 5198, or with that in the first spectrum (blue flame of Bunsen burner) at 5165. The measures on Oct. 7 were more satisfactory than those on Oct. 11 and 12, and are entitled to more confidence, both the comet-band and the carbon-band being much brighter. On Oct. 12 the carbon-band from the vacuum-tube (which was an old one) was hardly visible, being overpowered by the complex or band-spectrum of hydrogen which had, in course of time, replaced the ordinary hydrogen spectrum of four lines. The measures of the carbon-band were thus little more than guesses on this day. In the comparisons with the flame spectrum, it was difficult to avoid parallax, the slit being wide and the flame blown about by the wind.

The real difficulty in the determination of the exact position of the comet-band arises from the width of the slit, which is necessarily considerable. As far as these observations go, they would seem to show that different results are obtained according as the edge or the centre of the band in each spectrum is compared; and it is not easy to say which is correct, since the band (with a narrow slit) is sharp on the red side and fades off towards the blue. Further observations, however, are wanted to decide whether there is a real discordance as here indicated.

No measures could be made of the two other comet-bands. The band in the blue was only seen on Oct. 7, and Mr. Maunder then estimated that the band in the green was at least five times as bright as the two others.

Royal Observatory, Greenwich,
1880, *November* 10.

The July Aquariads 1880. By Edwin F. Sawyer, Esq.

The region situated within the boundary of the constellation *Aquarius* appears to offer attractions of more than ordinary interest to the meteor observer during the months of July and August, and especially at the end of the former month; an inspection of the Catalogues of various observers showing that many meteor tracks have been registered at this epoch, radiating from several quite well defined and apparently independent meteor streams centred in this constellation. Dr. Schmidt's observations of these meteors certainly extend back to the year 1849, for on July 28th and 29th of that year he found a position at R.A. $322°{\cdot}5 - 5°$, and his Catalogue, based on observations made between the years 1842 and 1868, contains no less than nine independent positions deduced during this epoch and situated in this region.

These meteors have also been frequently observed by Heis, Tupman, Neumayer, Weiss, Denning, Corder, and others. Mr. Denning found these Aquariads especially numerous at the end of July, in the years 1878 and 1879, furnishing meteor showers of considerable intensity (see the valuable paper by Mr. Denning in these *Notices*, vol. xl. No. 3).

My attention having been particularly directed to these meteors by this paper, a series of observations was arranged and quite successfully carried out by me this year. Watches were maintained on the evenings of July 28, 29, 30, 31, and August 1, and these Aquariads were found to be quite numerous, especially on the 29th and 30th.

The following table shows the number of hours spent in watching, and the number of meteors recorded :—

Date 1880.	Duration of Watch. h.	Length of Watch. h.	No. of Meteors seen.	Aquariads.	State of Sky, &c.
July 28	9 to 12	3	14	6	Clear; moon after 11·15 p. troublesome; meteo scarce. Hazy at tim and partly cloudy.
„ 29	9 „ 12	3	30	11	Meteors numerous.
„ 30	9 „ 12	3	36	11	Generally very clear, wi the exception of a slig haze after 11.30. M teors numerous.
„ 31	9 „ 11	2	10	2	Generally hazy.
Aug. 1	9 „ 11	2	12	2	Quite clear.
July 28–Aug. 1	9 „ 12	13	102	32	

Of the different magnitudes of brightness there were observed the following:—

= 1st	> 1st mag.	= 1st mag.	= 2nd mag.	= 3rd mag.	= 4th mag.	= 5th mag. and fainter.	Total.
2	5	18	13	18	19	27	= 102

On reducing the observations I was very much surprised to find that, instead of one radiant-centre furnishing the meteors from this constellation, there were at least three independent positions shown to exist; and the chief display was from a position at R.A. 330°—6°, this centre being quite accurately determined from several short paths near the focus, and giving a good radiation. Of the 32 Aquariads recorded 20 belonged to this stream. The shower R.A. 341°—13°, as deduced by Mr. Denning, and found by him to furnish the chief display, appeared quite inactive this year, although the position was feebly indicated by four short paths, and giving the centre as at R.A. 341°—10°. The third position was found to lie at R.A. 328°—15°, and was well determined from several very short paths accurately recorded near the focus. Eight paths altogether were recorded belonging to this position. The meteors from the chief radiant at 330°—6° were generally quite slow, of medium length and very bright, and from the other positions generally fainter and more rapid.

In looking over the various Catalogues accessible to me, several positions were found which appear to fully corroborate my results, the agreement in the majority of cases being quite close.

These positions together with mine are given below for comparison.

1st.

Denning,	July	16–25,	1876 = R.A.	337° 7°
„	„	5–17	1877 = „	336– 7
Heis	Aug.	21	1877 = „	336– 5
Sawyer	„	28	1880 = „	330– 6
Corder	„	29–30	1880 = „	331–10
			Average = R.A.	334– 7

2nd.

Tupman,	July	27–28,	1870 = R.A.	340°–14°
Denning	„	27–29,	1878 = „	341–13
„	„	28,	1879 = „	338–14
Schmidt,	„	20–31,	= „	340– 8
Sawyer,	„	28,	1880 = „	341–10
Corder,	July and Aug.		= „	337–13
			Average = R.A.	340–12

3rd.

Schmidt,	July 20–31,	= R.A.	333°–14°
Tupman,	,, 28,	= ,,	326–13
Sawyer,	,, 28–29. 1880 =	,,	328–15
	Average =	R.A.	329–14

These radiants also furnish many meteors during August and would appear to represent long enduring showers.

Cambridgeport, Mass., U.S.A.
1880, *Sept.* 30.

Mr. F. Cooper, in a letter dated Sheffield, Sept. 27, 1880, records an Outbreak on the Sun's surface, seen by him on Saturday August 14, while examining sun-spots with a power of 85 on a three-inch refractor, four feet focal length: at about $4^h\ 15^m$, G. M. T., he was scrutinising a small spot at the southern end of the group when suddenly, at an apparent distance of 10″ following, an intensely bright spot burst out, vanishing again before he had time to change the power. The colour of the Outbreak was a deep chrome yellow. He had no time to measure the diameter, but it was apparently about 3″. The coloured glass was a neutral tint. The Outbreak was much brighter than the Sun's disk, and the brighter and more deeply coloured of the faculæ, of which much was observable. At the time of observation the air was clear and still, no boiling, definition very good, the mottled appearance well and sharply defined generally over the Sun's surface. He inquires whether the phenomenon was seen by any other observer; or could it be merely an optical delusion?

As regards the magnetic condition at the time, Mr. Ellis writes that at Greenwich an active magnetic disturbance commenced 1880, Aug. 10, $22^h\ 30^m$, and continued till Aug. 11, 1? The magnets were quiet to Aug. 12, 0^h, and then very much d turbed till Aug. 12, 18^h; tolerably quiet till Aug. 13, 4^h, and th disturbed till Aug. 13, 18^h; afterwards and during Aug. 14 pretty quiet; very little motion then till Aug. 18. Previous Aug. 10, the magnets had been tolerably quiet.

Mr. Cromwell F. Varley, in a letter dated 2 Great W chester Street, Nov. 5, 1880 (accompanied by a sketch), gives account of an unusually fine Meteor seen by him the preced night, 3½ minutes to 10^h or $10^h\ 15^m$ while returning hom Bexley Heath, 11¾ miles E. on the main road. The m lasted one to one and a half seconds, being at its brightest than 12 times as bright as *Jupiter*, and it suddenly disapp near *β Orionis* (Rigel), leaving a brilliant green train lasted from five to six seconds; the colour and brilliancy o train resembled the voltaic arc taken between silver electr

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLI. DECEMBER 10, 1880. No. 2.

J. R. HIND, Esq., F.R.S., President, in the Chair.

Edwin Simpson-Baikie, Esq., F.L.S., F.R.G.S., 21 York Terrace, Regent's Park, and United University Club, Pall Mall East; and

Colonel Henry T. Rogers, R.E., Superintendent of Survey, Madras, 24 Bassett Road, Notting Hill, W.:

were balloted for and duly elected Fellows of the Society.

Plan for telegraphing Astronomical Discoveries.

(*From a letter of B. A. Gould to the Secretary of the Smithsonian Institution, 1880, Nov. 8 [with slight and unimportant modifications]*).

(*Communicated by the President.*)

I inclose the German astronomers' plan for telegraphing the discovery of Comets, Asteroids, &c., across the Atlantic. American observers are affected by it only as concerns the intelligibility of messages *received* by you. The Europeans seem of accord that they can receive most intelligibly messages sent in this way.

If you approve and adopt, please let me know at your early convenience, that the matter may be decided at once.

1. Only one despatch to be sent at a time. This, if for Europe, to be addressed to Prof. Krueger, at the Observatory in Kiel; if for America, to Prof. Baird, Secretary of the Smithsonian Institution. A second telegram to be sent as soon as a second observation has been obtained. Thus only two despatches in all.

2. All numerical data to be expressed by a word for each figure, without regard to its value as dependent upon position. Thus 58 to be telegraphed as "five-eight," 12 as "one-two," 30 as "three-zero," &c.

3. All places which would ordinarily be left vacant, when the number is written in figures must be filled up by the word "nought," or "zero." Thus for R. A.$=1^h\ 5^m{\cdot}3$ would be written "zero-one-zero-five-three," or for $19^h\ 3^m$ the despatch would be sent "one-nine-zero-three-zero" (the tenths of minutes being given for Right Ascensions, but not for other data, as by § 5).

4. The time of observation to be given in round hours of Greenwich mean time, preceded by day and month, and never in any other way. Thus an observation made on Oct. 12 at 13^h Greenwich mean time would be announced "One-two-October-one-three;" or if on Nov. 3 at $7^h\ 15^m$ Greenwich mean time, as "Three-November-zero-seven." In this way the time of observation, which should be given first of all, will be expressed by the name of the month preceded by one or two words, and followed always by two.

5. Right Ascension to follow the time of observation, and to be given by five words, expressing hours, minutes, and tenths of minutes in time, preceded by the word "ascension."

6. Declinations to be given by four words, expressing degrees and minutes, preceded by the word "North" or "South."

7. Daily motion in R. A. to be given in minutes and tenths of minutes of time, preceded by the word "plus" or "minus."

8. Daily Motion in Decl. to be given in minutes of arc, preceded by the word "North" or "South."

9. Magnitude, if given, to be preceded by the word "mag.," but expressed as an ordinal number.

Thus, in ordinary cases, the essential data will be comprised in twenty-three words, or twenty-five if the magnitude be given.

Example:—

Comet discovered by Swift Oct. 12. Place at 7^h Greenwich mean time R. A.$=2^h\ 23^m{\cdot}7$, Decl.$=+31°\ 2'$. Daily motion in R. A. $1^m{\cdot}2$ decreasing; in Decl. 8′ northward.

The despatch would be:—

comet swift one two october zero seven ascension zero t
two three seven north three one zero two minus one two no
eight.

Prof. Krueger (appointed editor of the *Astron. Nachricht* undertakes to repeat the despatch throughout Europe at the of the Kiel Observatory or of the Astronomische Gesellsc Prof. Foerster informs me that the Paris Observatory as w the German ones accept the arrangement. At Greenw assured myself of its acceptance there.

On a new Form of "Ghost" Micrometer. By Chas. E. Burton, Esq., and Howard Grubb, Esq.

In the present Note an arrangement for using the images of fixed or moveable lines in the measurement of images, formed by the objective of a telescope microscope or spectroscope, instead of the material lines hitherto generally employed, is described, and several methods of applying them are indicated.

It is well known that the ordinary wire micrometer is unsuitable for the measurement of two classes of objects, namely, planetary disks, and double stars which are near the limit of separability for the object glass employed when direct measurement is attempted. The difficulties attendant on its use in the cases above mentioned are briefly these:—(1) On bringing a material line in contact with the image of a luminous body of sensible magnitude, diffracted light appears on the side of the wire furthest from the image to be measured; (2) If a wire be placed on the image of a star, that image becomes elongated in the direction perpendicular to the wire. Defect (1) renders it extremely difficult to ascertain when a micrometer wire is accurately in contact, *e.g.* with the limb of a planet; and Defect (2), by reason of the interval between the components of an extremely close double becoming filled with light, when the wires are placed on the components, renders it impossible to obtain direct measures of such objects with the required certainty.

The difficulties above indicated have been treated very completely by Professor Kaiser in vol. iii. of the Leyden Observations, pp. 104–5, where he refers with approval to a proposal by Lamont (*Jahrbuch der K. S. bei München*, Seite 187) to employ "ghost" lines for such measurements; and by the Rev. W. R. Dawes, in *Mem. R.A.S.* vol. xxxv. pp. 153 and 161.*

The suggestions of Prof. Kaiser seem never to have been carried into effect.

In the *Proceedings of the Vienna Academy of Sciences* for 1856, vol. xx. p. 253, H. Karl v. Littrow describes a mode of forming a "ghost" of a system of wires applicable and actually applied to a meridional telescope. In this instrument the ghost lines are interrupted at or near their centres by the interposition

* Since this paper was read before the Society we have been enabled to examine Prof. Lamont's original paper, giving details of the construction of a form of ghost micrometer devised by himself, with which numerous observations of the satellites of *Jupiter* and the ring system of *Saturn* were made. The images of the lines (bright) or holes in a diaphragm were reflected by a glass plate nearly perpendicular to the end of the telescope, through which the stellar or planetary light passed. Professor Lamont's intention was (*loc. cit.*) to get rid of the errors of judgment which are almost or quite unavoidable when the extreme points of the line under measurement are concealed by the wires employed for that measurement. He does not mention the still more injurious effects of diffraction referred to by Mr. Dawes, and in paragraph (2) of this Note. Professor Stämpfer also employed a similar arrangement with much success for meridian work (*Sitz. Berichte Wien. Ak.* Bd. 20, Seite 314).

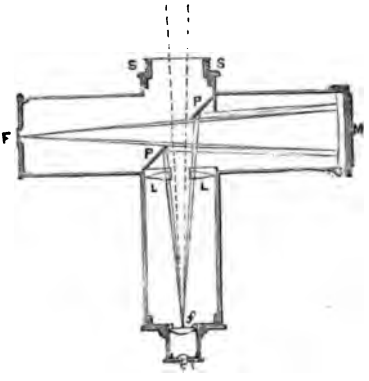
of an opaque bar, and the star is caused to traverse the series of blank spaces thus formed. It does not appear, however, that any attempt was made to adapt this to a position micrometer of an equatoreal instrument.

An instrument somewhat resembling that of Dr. Littrow, and designed for use as a position micrometer, was devised by Mr. G. P. Bidder, and is described by him in the *Monthly Notices* for June, 1874. In this form of ghost micrometer the image of the lines is formed by a unilateral oblique pencil of rays, and it would probably be difficult to avoid loss of definition due to dispersion by the lenses of the eyepiece, as well as to preserve an invariable scale value, on account of the want of a sufficiently rigid connection between the several parts of the instrument. Any movement of the second prism of the optical train, as proposed for the purpose of transferring the system of "ghost" lines to any part of the field, would directly tend to alteration of the scale value and introduce a variable parallax of serious amount.

The instrument now to be described is one of several forms contrived with the object of avoiding the difficulties encountered by those who have hitherto worked at this subject, and of extending the powers of the wire micrometer into the field occupied by the double image micrometer, by rendering the measurement of planetary disks, and close double stars practicable without the production of interference phenomena or the necessity of halving the light of the image, the last being a great hindrance to the use of the double image micrometer in the measurement of faint objects. At the same time the connection between the several parts of the instrument is so rigid and permanent that variation in the scale value will be produced by temperature only, and the corrections thereby necessitated will be of the same order with those already familiar to observers.

The accompanying figure represents the most simple form of the instrument hitherto devised by us, one-third of its actual size. Externally it consists of a pair of tubes, one in the axis of the telescope, and the other at right angles thereto. The first of these tubes is attachable to the draw tube by an ordinar "Tulley" screw at S, and carries at its lower end the eyepiec E. The cross tube carries at one end a "Reticule" or wi frame F, moveable or stationary, according to requirement, an at the other a cell containing a concave silvered mirror M, th radius of curvature of which is slightly greater than th distance between F and M. At the junction of the tubes mounted (at 45° to F M) a plane silvered mirror P, perfor by an elliptical hole slightly larger than is necessary to a the pencil of rays from the object glass to pass. Immedia below P is inserted a perforated achromatic lens L, to incr the convergence of the rays from M, and thus diminishin length of the eyetube while forming an image of F at the of the object glass. F being illuminated by a faint ex

light, the rays from it pass to M through the hole in P, and, being reflected by M in a cone of smaller angle, are received on the inner edge of P and are thereby diverted into the eyetube, where they are still further condensed by L and brought to a focus at *f*, the focus of the object glass.



In some cases the lens L may be necessary, and in others it may be found better to use a plane mirror at M and throw the whole work of forming the "ghost" images on the lens L. It is, of course, not necessary that the cross tube should be at *right angles* to the eyetube, and in some cases it may be advisable to use a different arrangement of the parts. If it should prove to be difficult to adapt the instrument to any existing telescope on account of its length (four inches), a Barlow lens can be inserted at S; but some forms of the apparatus have been devised in which there is no cross tube, and the "ghost" images are formed by a perforated concave mirror mounted in a tube capable of being pushed into the draw tube of the telescope. The best form of condensing lens L is under consideration, and it may possibly be found desirable to superachromatise it, to compensate for the undercorrection of the usual eyepieces for lateral pencils.*

In two of its simplest forms, including that represented in the figure, the "ghost" micrometer has been severely tested by one of us, as regards the effect of the images of the lines upon the images of various celestial objects furnished by an equatorially mounted objective of six inches' aperture and six feet focal length. The objects scrutinised for this purpose were the following: lunar details near the terminator and fully enlightened; *Jupiter* and *Saturn* with their satellites; and numerous stars, both single and double. In all cases the result was the same, not the slightest change of form or false light beyond the limb (of the planets) being visible, or even suspected, when the objective images were brought in contact with or were occulted (covered) by the "ghost" lines, powers up to 400 linear being employed whenever definition was suitable. The illumination of the reticules employed was extremely easy, and readily adjusted to suit the brightness of the object observed. It was possible with the direct light of an ordinary candle, unassisted by any optical arrangement for rendering it parallel or convergent, to increase the brightness of the lines sufficiently to make them clearly visible against the brightest parts of the

* Another form has since been devised in which this lens is not required.

Moon's image. The reticules employed were systems of lines cut through an opaque film deposited upon glass.

Suggested forms of "ghost" micrometer, with their applications.

(1) In its most simple form this micrometer will do duty as a transit eyepiece, displaying dark lines on a bright field, or *vice versâ*, and can be fitted with reticules of any kind for mapping groups of stars, or details of the Moon or planets, including of course circles for ring micrometer work.

(2) By substituting for the reticule a wire micrometer with one or two screws, the instrument can be used (*a*) instead of an ordinary uni- or bi-filar micrometer with bright field; or (*b*) as a unifilar micrometer with bright lines cut through an opaque film; or (*c*) as a bifilar micrometer with bright lines, the object lines (at F) being in this case spider webs illuminated by a Wenham paraboloid or other similar contrivance.

(3) By splitting the plane mirror P centrally in the plane M P E, and mounting one half on an axis perpendicular to that plane, attached at its upper end to a lever arm terminating in a sector actuated by an endless screw with divided head, a double set of "ghost" lines might be formed by inclining the two parts of the plane P to one another by a movement of the arm and screw. One of these sets of lines would be stationary, while the other moveable set would have its movement registered by the turns and parts of a turn of the endless screw. Of course the lines might be either dark on a bright field, or bright on a dark field, the latter being best, as in the former case the lines would not be black, but would be of half the brightness of the field. We have experimentally proved that the definition of the lines is unaffected by stopping off half the aperture of M or P in a direction perpendicular to them, and that their brightness does not sensibly diminish for a considerable distance past the centre of the field.

(4) By substituting diaphragms pierced with small circular apertures illuminated by polarised light variable in intensity and colour, this micrometer can be made available for photometric and colour comparisons of stars after the method of Zöllner. Not only can the field, in the case of bright field illumination, be illuminated with light of any desired tint, but if bright lines are employed they can be coloured as the observer may prefer, for there is always a superabundance of light which is yet completely under control. The lines can be shown "broken" or continuous, as may be desired. The first-mentioned arrangement may be advantageous under some circumstances.

A useful modification of the reticule mentioned in a form section would be a bright ring (photographed), divided in degrees and nearly of the same diameter as the field, which would assist materially in estimation of position angle when the faintness of the object viewed rendered *measurements* possible.*

* This suggestion has been anticipated by Dr. Lamont. *Vide su*

Lastly, when a slight loss of light is of no importance, a glass plate with plane and parallel sides, the preparation of which is a matter of no difficulty in the present state of practical optics, may be substituted for the plane mirror P, with the advantage of securing a large field, which may be surveyed by a low power eye-piece, or by one of high power so mounted as to be traversable over the whole field.

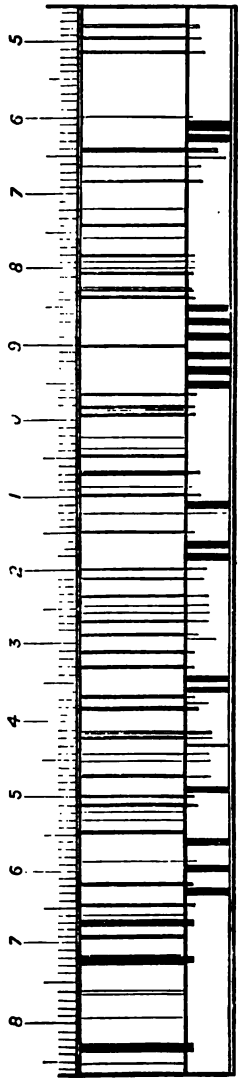
On the Spectrum of a Sun-Spot observed at the Royal Observatory, Greenwich, 1880, Nov. 27 and 30.

(*Communicated by the Astronomer Royal.*)

Amongst the spectra of Sun-spots which have been examined at Greenwich, that of the fine spot which was first seen near the east limb on Nov. 25 appears to be quite unique. When examined on Nov. 27, the most striking feature in the spectrum was a number of strong dark lines between *b* and F, to which there appeared to be nothing corresponding in the ordinary spectrum of the photosphere. Under favourable circumstances, however, faint lines were detected in the solar spectrum corresponding to all, or nearly all, of these dark lines in the spot, but none of these are represented either in Kirchhoff's or Ångström's maps, and they do not appear to correspond to any known element. The dark lines in the spot were fully as broad as b_1 or b_2, being about one tenth-metre in breadth, and somewhat fuzzy at the edges.

The following are their wave-lengths, as derived from careful comparison with the lines in Angström's map :—

5162·3	5118·2	5088·8
5159·7	5116·5	5086·9
5155·9	5111·0	5084·9
5148·9	5095·2	5062·7
5135·3	5093·4	5061·1
5134·0	5091·2	



The lines at 5129·4, 5112·1, and 5142·2 were much broadened in the spot; D and F were about one-third as broad again, and upwards of 300 lines between D and F were noted as being more or less affected.

On Nov. 30 the above-mentioned bands were not so marked, but the C and F lines were much broader (about doubled in breadth), and many lines between B and *b* were very much darkened and broadened. The *b* lines were hardly affected.

The observations were made by Mr. Christie and Mr. Maunder with the half-prism spectroscope, mounted on the Great Equatoreal. A train of two prisms, giving a dispersion of about 80° from A to H, was used, with a magnifying power of 10 on the viewing telescope.

On 1880 Oct. 1 a strong dark line at W. L. 5146·3, not corresponding to any line in Ångström's map, was seen in the spectrum of two Sun-spots. This dark line, though resembling in character those mentioned above, does not correspond in position with any of them.

The figure represents a portion of Ångström's map of the solar spectrum near *b*, with the spectrum of the Sun-spot below, showing the dark bands. In the case of the other lines the length represents the amount by which the line is broadened—full length corresponding to a line which is doubled in breadth; half length, to one of which the breadth is increased by one half; and so on.

Royal Observatory, Greenwich,
1880, *December* 7.

Telegraphic Determination of the Longitude of Shanghai.

By Dr. L. S. Little.

The completion of the submarine telegraph cable to Shang suggested to me that it was desirable to obtain the value of longitude by its means. Shanghai being the most important in China, an accurate determination is of importance to nav tion, even if I did not require it for my own Observatory and to correct the value I had obtained by occultations and culminations.

Shanghai is in telegraphic communication with Euro two routes—a southern, *viâ* Hongkong and India, and a nor *viâ* Japan and the Russian Empire.

The positions of several points in Japan were dete telegraphically by the various parties sent out to observe transit of *Venus*.

One of these points was the telegraph station at N and I am indebted to Prof. A. Hall, of the Naval Obser Washington, for the longitude of that point.

The telegraph station at Nagasaki is connected with that of Shanghai by

426 miles submarine cable Nagasaki to Gutzlaff,
57 „ „ Gutzlaff to Woosung,
9 „ land line Woosung to Shanghai.

In 1875 Vice-Admiral Ryder, Commander-in-Chief on the China station, directed Commander Dawson, of H.M.S. "Dwarf," with Lieutenants Bedford and Greaves, officers specially charged with surveying duties, to carry out the work at Nagasaki.

Lieut. Dreyer, of the Royal Danish Navy, Director of the Great Northern Telegraph Company in Shanghai, kindly placed the line at our disposal after ten o'clock at night without relays or breaks at Woosung or Gutzlaff.

The bell of Wheatstone's Morse instrument and the key of Wheatstone's (double current) instrument were used.

At Nagasaki local time was obtained by the observation of equal altitudes of the Sun by two observers independently. The observations consisted of from three to four sets of five each, both morning and afternoon, and were made in the yard of the telegraph office, where a mean-solar-time chronometer was kept during the week the work was going on.

At Nagasaki the weather was fine throughout.

At Shanghai local sidereal time was obtained by means of a fixed transit instrument and a marine sidereal chronometer beating half seconds. The Observatory is situate about a third of a mile from the telegraph office, and the chronometer was carried to and fro by hand.

Corrections of Nagasaki Mean Solar Chronometer, Dent No. 1505.

Date 1875.	Chronometer fast of Mean Time at Noon Bedford.	Greaves.	Mean.	Hourly Rate.	Difference between the two Observers.
	h m s	h m s	h m s	s	s
Nov. 1	3 6 32·738	3 6 34·087	3 6 33·250		1·149
				+0·2275	
2	27·535	28·260	27·791		0·725
				+0·2965	
3	20·426	21·427	20·676		1·001
				+0·2594	
4	14·310	14·590	14·450		0·280

It will be noticed that the difference between the two observers is much less on the fourth day, and this gives greater weight to the correction for that day, and to the longitude deduced from it.

Corrections of Shanghai Sidereal Chronometer, Mierendorff No. 10.

Day and Hour. 1875.		Correction.	Hourly Rate.
d	h	m s	s
Nov. 1	21· 0	+12 27·06	
			+0·046
2	2· 4	28·42	
			+0·077
3	21·70	29·91	
			−0·107
3	1·45	29·51	

In consequence of unfavourable weather, it was not possible to determine the instrumental errors on the first two nights as accurately as could be wished, but on the third night the errors were obtained more accurately.

The resulting longitudes determined on the three days of observation are exhibited in the following table, which gives the comparison of the values of the difference of longitude obtained from coincidences with that obtained by ten-second signals, omitting minutes.

Date 1875.	Wght.	Shanghai to Nagasaki			Nagasaki to Shanghai			Means adopted.		
		Coincid.	10ˢ sig.	Diff.	Coincid.	10ˢ sig.	Diff.	Coincid.	10ˢ sig.	Diff.
		s	s	s	s	s	s	s	s	s
Nov. 1	1	36·68	36·59	−·19	35·81	35·73	−·08	36·24	36·16	−·08
2	2	36·20	35·95	−·25	35·36	35·17	−·19	35·78	34·56	−·22
3	5	36·41	36·35	−·06	35·67	35·52	−·15	36·04	35·94	−·10
Means		36·39	36·28	−·12	35·61	35·52	−·15	36·00	35·87	−·13

This table shows that Shanghai observed systematically too late and Nagasaki too early by about the same amount, one-eighth of a second, assuming that in coincidences there is no correction required for personal equation.

The wave and armature time varies on the different days and with the different sets of signals from 0ˢ·37 to 0ˢ·44.

From the equations of condition formed from the wave and armature times and relative lengths of the cables given in Commander Green's telegraphic determinations of longitudes in the West Indies, the following values were found:—

x = armature time = $0^{s}\cdot2971$,

y = wave time for 100 miles = $0^{s}\cdot0175$ = 5700 miles in a second.

Applying this value of x to the mean wave and armature time obtained as above, $y=0^{s}\cdot0186$, or nearly the same value as that found by Commander Green.

The following table gives the resulting longitude of Shanghai East of Greenwich, as determined from these observations:—

	h	m	s
Wladiwostok, East of Greenwich, by cable	−8	47	30·91
Nagasaki, West of Wladiwostok, by cable	+	8	1·92
Nagasaki (telegraph office) East of Greenwich	−8	39	28·99
Shanghai (Observatory), West of Nagasaki	+	33	36·00
Shanghai (Observatory), East of Greenwich	−8	5	52·99

The latitude of the Observatory as determined by transits of stars on the prime vertical is

N. 31° 14′ 10″23.

Ephemeris for finding the Positions of the Satellites of Uranus, 1881. By A. Marth, Esq.

About the beginning of October 1881 the Earth will pass through the planes of the orbits of the satellites of *Uranus*, from the side on which the satellites appear to move in the direction of *de*creasing position-angles to the other side; it will repass the planes about the end of March 1882, and will, about the middle of July 1882, finally pass to the side on which the satellites appear to move in the direction of *in*creasing position-angles, and on which it will remain till the year 1923. The apparitions of the planet in 1881, 1882, and 1883 will consequently be most favourable for the determination of the planes of the satellites' orbits. The most important measurements for the purpose are those of the position-angles when the satellites are near their greatest elongations, and when they are very near the planet; and to secure these observations it may be advisable, in case there is not ample opportunity for observing, to dispense with getting exact measurements of the distances, and to devote the available time to obtaining greater accuracy in the position-angles. It has not yet become known how near to the planet the satellites may be observed with some modern instruments.

The angle of position p_o of the major axes, the major and minor semi-axes a and b of the apparent ellipses described by the satellites, and the latitude of the Earth above the assumed plane of their orbits, are the following:—

Greenwich Noon. 1881.		P_0	Ariel. a	Ariel. b	Umbriel. a	Umbriel. b	Titania. a	Titania. b	Oberon. a	Oberon. b	Lat. of Earth.
		°	′	″	″	″	″	″	″	″	°
Jan.	9	14·75	14·95	0·57	20·82	0·80	34·15	1·31	45·67	1·75	−2·20
	19	14·72	15·06	0·64	20·98	0·89	34·41	1·46	46·01	1·95	2·43
	29	14·68	15·15	0·72	21·11	1·01	34·62	1·66	46·30	2·21	2·74
Feb.	8	14·63	15·22	0·82	21·21	1·15	34·79	1·89	46·52	2·52	3·11
	18	14·58	15·27	0·93	21·27	1·30	34·89	2·14	46·66	2·86	3·51
	28	14·52	15·29	1·05	21·30	1·46	34·94	2·40	46·72	3·21	3·94
March	10	14·46	15·28	1·16	21·29	1·62	34·92	2·66	46·70	3·56	4·37
	20	14·40	15·25	1·27	21·25	1·77	34·85	2·91	46·60	3·89	4·79
	30	14·34	15·19	1·37	21·16	1·91	34·71	3·13	46·42	4·18	5·17
April	9	14·29	15·11	1·45	21·05	1·02	34·53	3·31	46·17	4·43	5·50
	19	14·25	15·01	1·51	20·91	2·10	34·30	3·45	45·87	4·62	5·77
	29	14·22	14·90	1·55	20·75	2·16	34·04	3·54	45·52	4·73	5·97
May	9	14·20	14·77	1·56	20·58	2·18	33·75	3·58	45·14	4·78	6·08
	19	14·20	14·64	1·56	20·40	2·17	33·45	3·56	44·74	4·76	6·10
	29	14·21	14·51	1·53	20·21	2·13	33·15	3·49	44·33	4·67	6·04

Longitudes of the satellites in their orbits reckoned from the points where they are at their greatest northern elongations :—

Greenwich Noon.		Ariel. Long.	Diff.	Umbriel. Long.	Diff.	Titania. Long.	Diff.	Oberon. Long.	Diff.
1881.		°	°	°	°	°	°	°	°
Jan.	9	228·23		218·61		15·11		9·39	
			1428·43		868·71		413·50		267·37
	19	276·66		7·32		68·61		276·76	
			·39		·70		·49		·36
	29	265·05		156·02		122·10		184·12	
			·37		·67		·48		·36
Feb.	8	253·42		304·69		175·58		91·48	
			·34		·66		·47		·34
	18	241·76		93·35		229·05		358·82	
			·31		·64		·45		·34
	28	230·07		241·99		282·50		266·16	
			·29		·62		·45		·33
March	10	218·36		30·61		335·95		173·49	
			·27		·61		·45		·33
	20	206·63		179·22		29·40		80·82	
			·25		·60		·45		·33
	30	194·88		327·82		82·85		348·15	
			·23		·60		·45		·34
April	9	183·11		116·42		136·30		255·49	
			·23		·60		·45		·35
	19	171·34		265·02		189·75		162·84	
			·22		·60		·46		·35
	29	159·56		53·62		243·21		70·19	
			·22		·61		·47		·36
May	9	147·78		202·23		296·68		337·55	
			·23		·61		·48		·37
	19	136·01		350·84		360·16		244·92	
			·24		·62		·48		·39
	29	124·25		139·46		43·64		152·31	

These values are to be interpolated for the times for w the positions of the satellites are required. The position-a p and distances s are then found by means of the formulæ—

$$s \cos (p_0 - p) = a \cos \text{long.}$$
$$s \sin (p_0 - p) = b \sin \text{long.}$$

The satellites move in the direction of decreasing posi angles, and will be at their greatest elongations ("N" in pos and "S" in posit. $p_0 + 180°$) and at their superior and inf conjunctions with the planet ("sup." in posit. $p_0 + 90$ and " in posit. $p_0 + 270°$) at the following hours, Greenwich time :—

Ariel.

1881	N.		S.			N.		
	d	h	d	h		d	h	d
Jan.	9	12·1	10	18·3	March	23	14·3	24
	12	0·5	13	6·8		26	2·8	27
	14	13·0	15	19·3		28	15·3	29
	17	1·5	18	7·8		31	3·7	
	19	14·0	20	20·2	April			1
	22	2·5	23	8·7		2	16·2	3
	24	15·0	25	21·2		5	4·7	6
	27	3·5	28	9·7		7	17 2	8
	29	16·0	30	22·2		10	5·7	11
Feb.	1	4·4	2	10·7		12	18·2	14
	3	16·9	4	23·2		15	6·7	16
	6	5·4	7	11·7		17	19·2	19
	8	17·9	10	0·2		20	7·7	21
	11	6·4	12	12·6		22	20·2	24
	13	18·9	15	1·1		25	8·7	26
	16	7·4	17	13·6		27	21·2	29
	18	19·9	20	2·1		30	9·7	
	21	8·4	22	14·6	May			1
	23	20·9	25	3·1		2	22·2	4
	26	9·3	27	15·6		5	10·7	6
	28	21·8				7	23·2	9
March			2	4·1		10	11·7	11
	3	10·3	4	16·6		13	0·2	14
	5	22·8	7	5·1		15	12·6	16
	8	11·3	9	17·6		18	1·1	19
	10	23·8	12	6·0		20	13·6	21
	13	12·3	14	18·5		23	2·1	24
	16	0·8	17	7·0		25	14·6	26
	18	13·3	19	19·5		28	3·1	29
	21	1·8	22	8·0				

Umbriel.

1881	N.		S.			N.		S.	
	d	h	d	h		d	h	d	h
Jan.	6	11·7	8	13·3	March	22	1·9	24	3·7
	10	15·1	12	16·8		26	5·4	28	7·2
	14	18·5	16	20·2		30	8·9		●
	18	22·0	20	23·7	April			1	10·6
	23	1·4	25	3·2		3	12·4	5	14·1
	27	4·9	29	6·6		7	15·8	9	17·6
	31	8·4				11	19·3	13	21·0
Feb.			2	10·1		15	22·8	18	0·5
	4	11·8	6	13·5		20	2·2	22	4·0
	8	15·3	10	17·0		24	5·7	26	7·4
	12	18·7	14	20·5		28	9·2	30	10·9
	16	22·2	18	23·9	May	2	12·7	4	14·4
	21	1·7	23	3·4		6	16·1	8	17·9
	25	5·1	27	6·9		10	19·6	12	21·3
March	1	8·6	3	10·3		14	23·1	17	0·8
	5	12·1	7	13·8		19	2·5	21	4·3
	9	15·5	11	17·3		23	6·0	25	7·7
	13	19·0	15	20·7		27	9·5	29	11·2
	17	22·5	20	0·2					

Titania.

	N. elong.		Inf. conj.			S. elong.		Sup. conj.	
	d	h		d	h		d	h		d	h
Jan.	8	15·2	Jan.	10	19·5	Jan.	12	23·7	Jan.	15	3·9
	17	8 2		19	12·4		21	16 6		23	20 9
	26	1·1		28	5·4		30	9·6	Feb.	1	13·8
Feb.	3	18·1	Feb.	5	22·3	Feb.	8	2·6		10	6·8
	12	11·0		14	15·3		16	19·5		18	23·8
	21	4·0		23	8·3		25	12·5		27	16·7
March	1	21·0	March	4	1·2	March	6	5·5	March	8	9·7
	10	14·0		12	18 2		14	22·4		17	2·7
	19	6·9		21	11·2		23	15·4		25	19·7
	27	23·9		30	4·2	April	1	8·4	April	3	2 6
April	5	16·9	April	7	21·1		10	1·4		12	5·6
	14	9·9		16	14·1		18	18·3		20	22·6
	23	2·8		25	7·1		27	11·3		29	15·5
May	1	19·8	May	4	0·0	May	6	4·3	May	8	8·5
	10	12·8		12	17·0		14	21·2		17	1·5
	19	5·7		21	10·0		23	14·2		25	18·4
	27	22·7		30	2·9						

Oberon.

	N. elong.		Inf. conj.		S. elong.		Sup. conj.
	h		h		h		h
Jan. 8	15·6	Jan. 12	0·4	Jan. 15	9·1	Jan. 18	17·9
22	2·7	25	11·5	28	20·3	Feb. 1	5·1
Feb. 4	13·9	Feb. 7	22·7	Feb. 11	7·5	14	16·3
18	1·1	21	9·9	24	18·7	28	3·4
March 3	12·2	March 6	21·0	March 10	5·8	March 13	14·6
16	23·4	20	8·2	23	17·0	27	1·8
30	10·6	April 2	19·4	April 6	4·2	April 9	13·0
April 12	21·8	16	6·6	19	15·4	23	0·2
26	9·0	29	17·8	May 3	2·6	May 6	11·4
May 9	20·2	May 13	4·9	16	13·7	19	22·5
23	7·4	26	16·2	30	0·9		

Observations of Meteors, Nov. 25–28, 1880.

By W. F. Denning, Esq.

Observations for the Andromedes or meteors of Biela's Comet were commenced here on Nov. 25, and partly continued until Nov. 29, but there were no active indications of the showers' return.

On Nov. 25 the sky was watched at intervals, but meteors were somewhat scarce. Only one was noted between $5^h\ 45^m$ and $6^h\ 15^m$, and that belonged to a radiant near η *Ursæ Majoris*. At $6^h\ 42^m$ a fine slowish meteor, with a thick spark-train, was seen shooting between α *Cephei* and δ *Cygni*, and its path conformed exactly to the radiant of the Andromedes. Several other meteors were recorded, but they were obviously directed from showers in *Taurus*, *Auriga*, and *Draco*.

On Nov. 26 a very remarkable meteor was seen at $6^h\ 14\frac{1}{2}^m$. When first observed it was passing 5° N.E. of ι *Ceti*, with a singularly slow motion, and its disappearance occurred 2° below ζ-η Aquarii. Length of path 31°. Directly I saw the meteor I began to count, and had enumerated 52 at its extinction. This I found by comparison with a seconds watch immediately afterwards to be equivalent to 15 seconds. The entire duration of the meteor must have considerably exceeded this, for it had already traversed a section of its course when it first came under my observation. It was the slowest meteor ever seen here. The radiant point was probably in *Eridanus* on the E.S.E. horizon. Only one meteor was observed during the evening with a path conforming with the radiant of the Andromedes, but observations were not of long duration.

On Nov. 27 the sky was observed during about three hours before $13^h\ 20^m$, and 24 meteors registered, but no Andromedes

Though on the two preceding nights this shower may have been very feebly visible, there was not, on the 27th, the slightest evidence of its return. A few fine meteors were seen belonging to two well-known streams of Taurids (at 63°+21° and 78°+24°): indeed, one-half the total number observed belonged to these showers, the former of which appeared to be the richer of the pair. I had never seen the Taurids I. so late before, though in 1876, Nov. 20, the shower was well observed here as a prominent display of bright meteors from the point 62°+22½°; and in the *Monthly Notices*, vol. xxxix. p. 23, I had pointed out the probable endurance of the shower until Dec. 6. As to the companion shower near β–ζ *Tauri* noticed this year, that also was well observed in 1876, both at the end of November and early in December, when 26 of its meteors were recorded. It was reobserved in 1877 by Messrs. Corder and Sawyer and the writer, and in 1878 Mr. Sawyer, during a series of observations for the Andromedes, found this the chief shower visible, and determined its radiant at 81°+21° (9 meteors). Mr. Greg designated this stream the Taurids II., in order to distinguish it from its rival and apparently contemporary shower near ϵ *Tauri* (the Taurids I.). It evidently forms a display above the average strength, and endures from Nov. 22 to Dec. 12 without apparent cessation. The following are the observations of this stream which have been made since 1876:—

		α	δ		
1876	Nov. 22–Dec. 8	80°	+23°	26 / s	Denning,
1877	Nov. 25–Dec. 13	81	+25	6 / s	Denning,
1876–9	December	78	+23	20 / s	Corder,
1877	Nov. 30–Dec. 9	80	+22	10 / s	Sawyer,
1878	Nov. 26–29	81	+21	9 / s	Sawyer,
1879	November 27	78	+24	5 / 's	Denning,
	Dec. 7–13	83	+23	27 / s	Denning (Foreign Obs.)
[1869	December 11	82·9	+22·9	stationary /	Bartel (Brünn)],

giving an average position at 80°·5+23°·0 from the eight observations.

The largest meteor seen this year on Nov. 27 was a Taurid at $9^h\ 2^m$ brighter than *Jupiter*. It traversed nearly the whole of its path as an ordinary shooting-star less than first magnitude, but suddenly burst out, at the end-point, into a brilliant flash, where it left a short streak of about ½° for some 5 seconds. At $12^h\ 29^m$ a stationary meteor was noted in 78°+24°. It was evidently approaching the observer in a sinuous course, so ♌, and appeared in the diverging point of the Taurids II.

On Nov. 28 observations were renewed, but there were no Andromedes, though the showers of Taurids continued in active progress. The following were the most remarkable shooting-stars registered on the several nights of observation:—

Paths of Shooting Stars. November 25-28, 1880.

No.	Date. 1880	G. M. T. h m	Mag.	Path from α δ	Path to α δ	Length of Path.	Notes.	Radiant Point.
1	Nov. 25	6 42	1	319°+58°	289°+51½°	19	1½ sec. Bright train.	γ Androm.
2	26	6 14½	2	6−7½	335−4	31	15 sec.; yellow; very slow. Died out in a faint spark.	Eridanus
3	26	6 52	2	27+54	26+60	6	Slow.	? γ Androm.
4	27	7 36	1	21−9	9−17	14	3 sec.; very slow; sparks.	Taurid I. or II.
5	27	9 2	>4	57+67	38+82	15½	Bright flash and streak at the end.	Taurid I.
6	27	11 13	1	27+16	14+12	13	Slowish.	Taurid I.
7	27	12 16	2	15+24	350+20	23½	Slow.	Taurid I.
8	27	12 29	2	18+5	7−2	13	Bright train; on horizon.	Taurid I. or II.
9	27	12 29	3	78+24			Stationary; 1½ sec.	Taurid II.
10	27	12 38	2	54−9	52−16	7	Short and swift	Taurid I.
11	27	12 45	1	86+56	95+70	14½	Not swift; train.	Taurid II.
12	27	13 0	>1	211+41	229+41½	13	Slowish; on horizon.	?
13	27	13 15	3	49+25	45+10½	15	Very very swift.	η Ursæ.
14	27	13 20	4	54+16	49+14	5	Slowish. Foreshortened near radiant.	Taurid I. or II.
15	28	7 33	1	336+55	290+39	34	Slowish.	Taurid I.
16	28	12 6	2	343+36	333+32	9	Rather swift.	Taurid II.

At $10^h\ 59^m$, Nov. 27, two small meteors appeared simultaneously, in the Lynx, with paths intersecting thus :—

The Meteors of November 14, 1880, *at the Observatory of Moncalieri* (*Italy*). By Signor P. F. Denza.

Fogs and clouds have almost completely prevented the observations we intended making at this Observatory on the nights which elapsed from the 12th to the 15th inst., in order to follow the phenomena which at this period usually present themselves.

On the morning of the 14th, the clouds and afterwards fogs began gradually to dissipate themselves at about 4 o'clock A.M., and by a short time before 5 the sky got clear and favourable enough to make some good observations.

Then we began to work, and from 5 to a quarter to 6—that is, until the increasing daylight obliged us to withdraw—we paid attention to the exploring of the sky.

The results we obtained in so short a time were very satisfactory. Four observers reckoned 37 meteors in three quarters of an hour, making about fifty meteors an hour—that is, more than twelve meteors for each observer, while last year we reached but four or six at the utmost.

More than the third of the observed meteors—that is, thirteen—belonged to the groups of the Leonids.

They all radiated from the celestial region placed in the scythe of *Leo*, the middle position of which is—

$$\text{R. A.} = 147^\circ, \qquad \text{N. Decl.} = 23^\circ,$$

which is almost the same position the English observers found last year—that is, that one of the usual point of radiation of the great current of the Leonids.

Another well-defined shower was observed from *Ursa Major*. Seven meteors accurately determined gave for the radiant of this shower the point—

$$\text{R.A.} = 157^\circ, \qquad \text{N. Decl.} = 46^\circ,$$

which is found near the stars λ and μ of *Ursa Major*.

The meteors of these two groups, and especially those of *Leo*, were amongst all the most beautiful. Of the thirteen Leonids, in fact, two were superior to the first magnitude, two of the first, four of the second, four of the third, and only one of the fourth magnitude.

And amongst the seven of the second group, one surpassed the first magnitude, two were of the second magnitude, and the remaining four of the third or fourth magnitude. No one of the remaining meteors reached the first magnitude.

In the Leonids the red and bluish colour predominated; the colours of the others varied between white and blue.

Several of the first were followed by a luminous ray, and almost all were seen as usual in groups of two or more at a time.

Amongst all the meteors we observed, the most splendid was

the Leonid which appeared at $5^h\ 23^m$ in the constellation of *Hydra*. Its nucleus, larger than *Jupiter*, was refulgent with a bluish light, and left behind it a bright streak of the same colour. The extremes of its course in the celestial region were—

At the beginning	R. A. = 131°,	S. Decl. = 3°,
At the end	R. A. = 124,	S. Decl. = 18.

Our observations, although they were few and of a short duration, nevertheless confirm those made last year in some places in England and in America, and show that the Leonids have in these late years acquired a greater vigour, and that we have thus traversed a space of the meteor current which gave us the grand results of the years 1866–68. The other Italian observations, which have not yet reached us, might perhaps give more weight to our conclusions.

The zodiacal light presented itself with great splendour to the East, on the clear horizon, rising up with its vertex as far as the tail of *Leo*.

Moncalieri Observatory,
1880, *December* 5.

The Leonids, 1880.

(*Extract of a letter from Mr. E. F. Sawyer, Cambridgeport, Mass., to Mr. Denning.*)

"I was on the look-out for Leonids on Nov. 11 and 12 (the 13th and 14th being overcast), and observed 12 as follows:—

"Nov. 11.—Watched from $14^h\ 30^m$ to $16^h\ 30^m$ (two hours), with perfectly clear sky, and recorded 14 meteors, of which 6 were Leonids.

"Nov. 12.—An hour's watch only was obtained in a sky more than half overcast, from $16^h\ 15^m$ to $17^h\ 15^m$; 8 meteors were noted, 6 being Leonids. The rate was at least 15 Leonids per hour, in a clear sky, for one observer. My centre of observation was in *Leo*, and my view was restricted to about one-sixth of the visible heavens.

"The Leonids were bright, 1 ⟍ 1st mag. and 2=1st. mag. They were short, rapid meteors, with streaks as usual.

"I have received the following note from Mr. Isaac Sharples, of Haverford College Observatory, State of Penn., U.S., regarding this shower. He says, 'Between $3^h\ 15^m$ and $4^h\ 15^m$ A.M. Nov. 13, Phil. mean time, 52 meteors were observed and mapped, of which 28 were Leonids. Four of them were as bright as 1st mag. stars, but the majority were faint. Many left streaks behind them. At the end of the watch, when the sky became cloudy, meteors were coming at the rate of two per minute. There were four observers.'

"This would certainly show that the Leonid meteor-shower was well sustained this year, and doubtless represents the perihelion passage of the minor cluster of 1847."

Note with respect to the Rate of Motion of Gaseous Matter projected from the Sun. By A. C. Ranyard, Esq.

Prof. Young, in the November number of the *American Journal of Science*, describes an exceptionally large prominence which he observed on the morning of October 7 last. He states that at 11 A.M. he saw a bright horn on the S.E. limb of the Sun. "When first seen, it was about 3′ or 4′ in elevation, but it rapidly stretched up and before noon reached a measured altitude of over 13′ (350,000 miles+) above the Sun's limb. It faded away and disappeared about $12^h\ 30^m$. It was brightest about $11^h\ 30^m$ with an altitude of about 8′."

If we suppose that the outburst was directed vertically upwards from a part of the Sun's surface near to the solar limb, so that there was no foreshortening, this observation would prove that the gaseous matter of the prominence was carried from a height of about 212,500 miles above the photosphere to a height of over 350,000 miles in less than thirty minutes. Comparing the rate of upward motion of the gaseous matter of the prominence with the velocity of a projectile thrown vertically upwards in free space to a similar height above the Sun's limb, we find that there is ample evidence to show that the matter of the prominence must have encountered considerable resistance, which caused it to pass over the distance traversed more rapidly than it would if it had moved freely under the action of solar gravity in empty space. Thus, since the time occupied by a projectile in passing upward from any level to the summit of its trajectory is equal to the time occupied by a body in falling under the influence of gravity from the higher to the lower level, we have —taking the Sun's radius as 425,000 miles, and the value of gravity at the level of the photosphere as ·169, where a mile is the unit of length and a second the unit of time—

$$\frac{d^2x}{dt^2} = -\frac{\cdot 169 \times (425000)^2}{x^2},$$

where x is the distance in miles of the falling body from the Sun's centre, and

$$\left(\frac{dx}{dt}\right)^2 = V^2 = \frac{2 \times \cdot 169 \times (425000)^2}{x} + c.$$

If the body drop from a height a miles above the Sun's centre, we have $V=0$ when $x=a$;

$$\therefore c = -\frac{\cdot 338 \times (425000)^2}{x},$$

and

$$\left(\frac{dx}{dt}\right)^2 = \cdot 338 \times (425000)^2 \left(\frac{1}{x} - \frac{1}{a}\right);$$

$$\therefore 425000 \sqrt{\frac{\cdot 338}{a}}\, \frac{dt}{dx} = \frac{x}{\sqrt{ax - x^2}};$$

which gives

$$425000\sqrt{\frac{338}{a}}t = \sqrt{ax-x^2} - \frac{a}{2}\sin^{-1}\frac{2x-a}{a} + c;$$

when $t=0$, $x=a$

$$\therefore c = \frac{a}{2}\frac{\pi}{2};$$

hence we have

$$425000\sqrt{\frac{338}{a}}.t = \sqrt{ax-x^2} + \frac{a}{2}\cos^{-1}\frac{2x-a}{a};$$

and for the time of falling from a height of 775,000 miles to a height of 637,500 miles above the Sun's centre we have

$$425000\sqrt{\frac{338}{775000}}.t = \sqrt{(137500)(637500)} + 387500\cos^{-1}\frac{500000}{775000},$$

which gives

$$t = 2256 \text{ sec}$$
$$= 37^{m}\ 36^{s}.$$

Hence the matter of the prominence must have passed over the lower part of its course with a considerably greater velocity than would have been necessary to carry a projectile unimpeded by a resisting medium to the observed altitude above the photosphere attained by the prominence matter. It seems evident from Prof. Young's description that the matter of the prominence was not carried in a non-luminous condition to an altitude which exceeded the observed altitude, for the gaseous matter composing the higher part of the prominence seems to have been so brightly incandescent that it was more than half an hour in fading from view.

But the observed altitude may have corresponded to a much greater height above the photosphere than 350,000 miles, for the prominence may have been thrown up from a part of the Sun on this side or on the other side of the limb. The calculated time consequently corresponds to a minimum estimate of the time which would have been occupied by a projectile unimpeded by a resisting atmosphere in traversing the space between the observed altitudes.

If on a future occasion the time which the erupted matter occupies in attaining various altitudes can be carefully observed, some valuable deductions may no doubt be drawn with respect to the distribution of the resisting medium which observations made during total eclipses had already shown to exist in the region of the Corona.

The photographs taken during the Eclipse of 1871 show that the tree-like and curving structures observable on that occasion were more contorted in the lower than in the higher regions of

the Corona, but the evidence with respect to the structures which have been visible in other coronas is not sufficiently full to allow of similar generalisations.

The fact that the light derived from the bright structures of the Corona as observed during total eclipses is strongly polarised shows that the brightness of the structures is not chiefly due to incandescent gas. It seems probable that the erupted matter must be quickly precipitated by the cold of space, leaving clouds of dust, which disperses the light derived from the photosphere. The strong polarisation of the light from the bright coronal structures seems to show that the precipitation gives rise to particles which are small compared with the wave-length of light. It is possible (as has frequently been suggested) that showers of metallic rain may also be produced, but the greater part of the light of the structures is evidently due to the light dispersed by the small non-incandescent particles.

Note on the Length of the Pendulum observed by De l'Isle de la Croyère at Archangel in 1728. By Major J. Herschel, R.E., F.R.S.

The observations at Archangel were made by Louis de l'Isle, brother of the geographer, William, and were recounted and communicated to the Academy of St. Petersburg by another brother, Joseph Nicholas, the astronomer, some years later—*i.e.* in 1735.

Their position in the series of pendulum observations should have made them of great importance, but two circumstances conspired to make them valueless. Of these, one was the delay in making them known; the other is the subject of this Note. A few words about the first may perhaps not be out of place.

The only pendulum observations which had obtained any repute were the following:—

1. 1670–1674, by Picard; at Paris, Uranibourg, La Haye, Lyons, and Cette;
2. 1671–1672, by Richer; at Cayenne and Paris;
3. 1681–1682, by Varin and Des Hayes; at Goree and Guadaloupe;
4. 1686, by Goüye; at Louveau (a place once the capital of Siam, but unknown to modern maps);
5. 1697–1698, by Couplet; at Paris, Lisbon, and Parayba;
6. 1699–1700, by Des Hayes; at Cayenne, Grenada, St. Christopher, and St. Domingo;
7. 1704–1705, by Feuillée; at Porto Bello, and Martinique.

Mention is sometimes made of a few others, but there are no authentic accounts; nor, indeed, are these, with one exception, of any present value except as historical indications of a certain kind. There is no trace (that I have met with) of any observation later than Feuillée's, until we come to De l'Isle's in 1728.

It is further noteworthy that in no case are the early observations described in detail, in a way to permit of any check upon the result. In general the observation consisted of lengthening or shortening the line by which a bullet or ball was suspended, until its swings were sensibly synchronous with those of a clock pendulum. The length is then recorded, and in most cases the length so determined is erroneous to an extent which can certainly not be attributable to error of observation in the ordinary sense. I attribute it only partly to large unsuspected variation of clock-rate during the diurnal period. Such variation is by no means incompatible with a fairly uniform daily rate; and that daily rates of a really startling uniformity *were usual* in those days is abundantly in evidence: see, for instance, Richer's observations at Cayenne, only sixteen years after the invention of pendulum clocks.

The account of De l'Isle's observations, given at considerable length, is therefore the first of its kind, and is especially interesting for that reason—at any rate to us; though, as I have said, the delay in publication no doubt made it sink into insignificance by the time it reached Paris, where in 1735 the elaborate observations of Mairan were no doubt occupying attention. And I think it probable that the interest which might have attached to it on account of the high latitude of the station was destroyed by the near expectation or actual possession shortly afterwards of those obtained by Maupertuis in 1737–8 at Pello and Paris.

But for these untoward conditions, I imagine the result obtained at Archangel would have received more notice. As it was, the French *savans* paid no attention to it; but it was considered by Krafft in his Essay on the Figure of the Earth, in 1736 (published 1741).*

I need hardly say it has been noticed by all modern writers. It was not, however, among the fifteen selected by Laplace, but it is included among the "second class" observations arranged by Airy, where its apparent error catches the eye as being much the largest. One of the objects of this Note is to rescue it from that invidious distinction.

To come to the point: the writer of the account has made a curious (but patent) blunder in deducing from the actual observation of *rate* the *linear* result which custom then and always has prescribed. I will quote his words:—

"Longitudinem penduli simplicis constat esse in ratione inuersa radicum quadratarum numeri oscillationum tempore æquali peractarum."

And he proceeds to apply this as follows. The length being

* Since this paper was written I have become acquainted with a very little known review of the early history of pendulum inquiry, viz. that of De Brémond, printed (as a note to his translation of the *Philosophical Transactions of the Royal Society*, for 1733 and 1734) in 1740. I hope to say more about this somewhat remarkable review elsewhere; for the present it will suffice to say that De Brémond falls into De l'Isle's error quite naturally.

440·5 lines, the pendulum made 28815 oscillations in 8 hours—*i.e.* in 28800 seconds. Therefore, the length must be reduced in the proportion of the *square root* of 28800 to the *square root* of 28815; whence a reduction of ·115 upon 440·5.

But the proportion should be as the *squares*, not as the *square roots*; and the reduction should therefore be four times as great.

Following out the calculation, and making the necessary arithmetical corrections, without introducing modern changes, I find: that the difference of length between Paris and Archangel, which the author determines as between the extremes ·163 and ·144 line, or about three-twentieths, should have been between ·507 and ·430, or nearly half a line.

But the result is better seen by noting that the seconds pendulum is determined as being between 441·007 and 440·930 lines, without reduction for atmospheric effects—say 440·97. Now, the allowance for air would be, with such a ball as was used on this occasion, about +·12, making the concluded length equal to 441·09.

It is natural now to inquire whether this result is in accordance with what we know of the Earth's surface. To test observations expressed in this way, I am in the habit of applying formulæ which assume

(1) That the seconds pendulum at the equator is 991 millimetres in length, with a symbolical correction.

(2) That the compression is $\frac{1}{290}$, with a symbolical correction.

(3) That "Clairaut's fraction" * is therefore ·00522, with a symbolical correction.

The length of the seconds pendulum in latitude λ will therefore be, in millimetres,

$$991(1 + \cdot00522 \sin^2 \lambda) + \text{symbolical terms.}$$

To find the length in inches or lines we must substitute 39·0161 or 439·306 for 991.

Now, the latitude of Archangel is 64° 32′; and therefore $\sin^2\lambda = \cdot8151$; whence we may find the length of the seconds pendulum in the latitude of Archangel to be—

$$439\cdot306 + 2\cdot2932 \times \cdot8151 = 441\cdot175 \text{ nearly.}$$

De l'Isle would seem, therefore, to have been only ·09 of a line in error, supposing my assumptions justifiable, and that there is no local disturbance at Archangel to account for it. A disturbance of that magnitude would be large, but not incredible.

I have already written at greater length on this not very

* This is the name given by Mr. Todhunter to the ratio of the increment of gravity from equator to pole to the force of gravity at the equator. The word 'therefore' in the text refers to Clairaut's Theorem.

important rectification than the occasion may seem to need. The truth is, that it has other aspects. It is, indeed, pleasant to rescue a good determination from an undeserved reproach; and it is curious to remark how the very obviousness of the mistake —like Poe's "Purloined Letter"—has allowed it to pass unnoticed. But it is rather as a justifying example of the need of sceptical criticism that I adduce it.

At the same time I would not be understood to hold the opinion that any gravimetrical observation that ever was made is valueless merely because it cannot compare with modern ones. The comparison is rather with what is possible according to experience. Suppose that a local disturbance of ·05 line (or 5" per diem) is admissible: then why should we scorn an observation against which nothing can be said except that it ranges between ·03 and ·06 line, merely because so large a range is nowadays unusual? In the present case there is a more cogent objection, for not only is the range (*i.e.* the probable error) large, but also the mean denotes a disturbance which is greater than any others at present known *in that region*. All the same, it cannot be rejected summarily, like a great many others I could name, as being certainly quite wrong.

There is a work by the same author entitled *Histoire de l'Astronomie*, published at St. Petersburg in 2 vols. in 1738, which I believe reviews all the early observations with pendulums; but I have hitherto been unable to obtain it—neither of the libraries at the British Museum nor at Burlington House possessing the work—and I am curious to know whether the error above indicated is reproduced there.

Collingwood,
1880, *November* 25.

Saturn in October 1880. By W. F. Denning, Esq.

Towards the end of September and in October, when the planet came to opposition with the Sun, I made a number of observations of his physical appearance as revealed by a Browning—with reflector of 10 inches' clear aperture, powers 450 and 500, which I usually found best adapted to the work. The planet was carefully scrutinised on Sept. 21, 27, 30, Oct. 2, 11, 12, 14, 18, 20, 23, 29, 30, 31, Nov. 2, 3, and 4, and the appearances presented were generally the same, with this obvious difference, that on the nights of best definition more details were glimpsed than on less favourable occasions. My observations were usually conducted for five or six hours, during the whole of which attention was divided between the planets *Saturn* and *Jupiter*.

The telescopic aspect of *Saturn* may be thus described:—

I. *The Ball.*—The equatoreal zone is the brightest region of the disk, and gives indication of white patches in moments of

best definition. South of the equator there lies a faint, somewhat broad, dusky belt, in which no bright or dark spots are distinguishable; and next to this a slightly lighter interval, followed again by another belt, fainter than that just described, and displaying a very similar aspect. Another minute band of shading outlies this, and, further south, this is bounded by a very light zone, which contrasts greatly with the dusky bands. Where this bright belt comes up to the margin of the disk, it apparently projects beyond the limb, so as to give the impression of bright patches there, and causing a most striking distortion of the globe. Yet further south of the bright belt there come a series of very dark belts. The one immediately contiguous to the pole exhibits the greatest intensity of shading, and is, in fact, the darkest region of the disk, and there is a fainter band dividing this from the light belt. At the pole itself I at first suspected a bright cap, but renewed observations, made on several nights when the air was exceptionally steady and the planet free from the tremulous motion which so effectually destroys good definition, showed that the immediate pole, though lighter than the dark belt surrounding, was much enveloped in shade. North of the equator, and just skirting that part of the crape ring projected on the ball, is another faint belt, in which I can make out no variations of shading, nor are any distinct markings visible on the north segment of the ball, though it is much darker than the ring generally.

The divisions between the triple belt lying south of the equator, above described, were only observed on the best nights. As a rule, the belts were merged into one very broad band of shading, faintest at its south extremity.

II. *The Rings.*—The outer ring A is somewhat faint, especially at its extreme outer limits, and can be traced across the planet as a narrow bar of shading. Encke's division is not equally well observed at all times. Sometimes I see it as a pencil line only curving round the extremities of the major axis of the rings; but in the best moments it comes out plainer as a dark and distinct division. This was particularly noticed on Oct. 29 and Nov. 4. Ball's division was always very plain. The rings A and B where they skirt this division were much brighter than in other parts. On the ring A I frequently suspected faint lines of shading, but the slightest rippling or vibration prevented the observation of these delicate features, so that they never remained long enough in view to place their existence beyond a doubt. The brightest part of the system of *Saturn* is unquestionably the outer region of B, which is certainly brighter than the equatoreal light-zone. The interior part of B is shaded off until near the margin of the crape ring, where it apparenly recovers a little of the brightness which characterises its outer portions. Dawes and several other observers have represented the interior shading of B as gradual, but it always appears to me

as being very decided and abrupt, and to show a faint pencil-line where it begins. Further on its interior the depth of shading is greater, though the extreme inner border becomes lighter again, perhaps by contrast with the darkness of the crape ring. The crape ring C appears certainly brighter on its inner margin than where it adjoins the luminous vein of B, but this also may be due in a measure to the effects of contrast, for the inner edge of the ring is bounded by dark sky, while the outer margin is against the light rim of B. I have seen no certain division in the crape ring, but have recorded the extreme darkness of its outer border, which may amount to the same thing.

No spots or irregular markings were traced upon the ball or the rings, except, perhaps, some bright patches on the equatoreal region and at the limbs on the bright belt near the south pole. The belts, though extremely faint, could be traced nearly right up to the margin of the disk, in this case offering a distinct contrast to the belts of *Jupiter*, which are obliterated in coming up to the limb. Moreover, while the equator of *Saturn* is the brightest zone of the disk, that of *Jupiter* shows the greatest depth of shading.

The night of Oct. 29 was one of the best of the season. Between 10^h and 11^h the details of the planet were presented with magnificent distinctness. Powers up to 600 could be employed with effect, and even with 750 the view was fairly good, though I found that nothing was gained by the employment of anything higher than 450.

On Oct. 31 the belts were very plain between 7^h and 8^h—the one nearest equator evidently darker than the others; and there were varieties in tone near the margin.

On Sept. 21 the dark space between the ball and crape ring looked larger on the east than west ansa, and slight differences were noticed on other nights, though not always of the same character.

But by far the most striking feature in the telescopic appearance of *Saturn* was the *curious figure* of the globe. The south pole, over which the dark belts lay, seemed *compressed* in the most remarkable manner; and where the bright belt intervened, the contrary effect was produced. Here the limb was apparently raised above the circular contour, so that the distorted image gave the planet that distinctly "square-shouldered" aspect which has been occasionally mentioned as a singular anomaly in observations of Saturn. The figure was on several recent occasions, when definition could hardly be improved, so clearly manifested, that the planet could not have been attributed a truly spherical form unless the observer had known it to have been unequivocally demonstrated by micrometric observations.

The explanation appears to me very simple. The singular figure is due to the contrasting effects of the belts. While the bright belt near the pole causes a very evident "shouldering" out of the two limbs at its extremities, the dark belts at the pole and

towards the equator act in a precisely contrary way. They apparently compress the planet where they come up to the limb, and thus the distorted figure is originated. I tried to get rid of this by very careful focussing, and thought that in transient intervals of superb definition the true spheroidal form of the planet would come out; but this was not the case. The singularity of figure was constantly maintained; so as, in fact, to form one of the most prominent features in the telescopic aspect of the planet; though it was not always equally decided, for at the end of October and on the first few nights of November it was not nearly so conspicuous as during some of the earlier observations.

Observations of the Great Southern Comet I (1880) *made at Castlemaine, Victoria.* By William Bone, Esq., M.D. (*Communicated by the Secretaries.*)

I take the liberty of forwarding a memorandum of my discovery of the Comet of February last, together with my measurements.

I gave Mr. R. A. Proctor a rough memorandum of the same when here recently; but since then I have taken a fresh series of observations for ascertaining truthfully my *micrometer values*, which has altered the interpretation of the observations in declination.

For the care and accuracy of the actual measures I can now with confidence vouch, having waited until the stars came round again, and verified every position. I offer them as an amateur's effort.

1880. Feb. 2. Discovered.—At 7^h 30^m on the evening of Feb. 2 last, on going to my Observatory (in company with the Rev. E. Crawford, Church of England priest of this town), I noticed a peculiar band of light near the position of the Sun, which had recently set. At first I thought it was a shoot of light from one of the foundry furnaces, but its stability soon convinced me it could only be the tail of a Comet. As the light of day waned, this was quite apparent. The nucleus was below the horizon, and must have been very close to the Sun. The tail extended obliquely upwards for about 35°, near *β Gruis* and towards *Nebula Minor*. A careful watch was kept from this time. I then telegraphed to Mr. Ellery at the Melbourne Observatory, and sent daily reports as long as the Comet was in view; but he was not fortunate enough to see the nucleus for several days after I had measured its position. There was no sign of the Comet before sunrise on the following morning. My little Observatory was besieged.

Feb. 3.—The Comet's nucleus was still below the horizon.

The tail was a little S. of S.W., and had perceptibly travelled East and North. It was not so bright as on the previous evening, and from that circumstance and its receding from the Sun, I surmised it had passed its perihelion point.

Feb 4. Nucleus first seen.—On this evening the sky was partially obscured by a few loose cumulo-strati. At $8^h\ 45^m$ G. M. T. they cleared off, and I had about ten minutes' distinct view of the nucleus. It was near the horizon, and an intervening building soon destroyed my view. During the short view I had I could clearly see a "vacuole" and the reflected (to use an anatomical term) envelopes, similar to the drawings of Donati's Comet.

Feb. 5.—The approximate positions shown by the Circles of the Equatoreal (Z. Gaunt, Melbourne, with $4\frac{3}{4}$-inch Wray object-glass) were—

	h m s		° ′ ″
R. A.	22 28 0,	Decl. S.	33 35 0,

	h m s
Time	7 30 0.

Fearing to lose my view speedily, I had arranged an Altazimuth on the top of a hill near, 200 feet above my level, and the position of the Comet's nucleus at $8^h\ 58^m$ was *exactly four degrees* in azimuth south of *Fomalhaut* and between 1820 and 1816 La Caille *Piscis Australis*, having *the same Right Ascension exactly as* 1820.

Feb. 6.—The nucleus was again seen, but when found was too low down for the Equatoreal.

Feb. 7 and 8.—Sky overcast. No observations.

Feb. 9.—Clear. Micrometric observations were obtained from μ *Sculptoris*, the Circles giving Comet's approximate place—

	h m s		° ′ ″
R. A.	23 37 11,	Decl. S.	33 30 0,

	h m s
Mean of Times of Measurement	9 2 30 G. M. T.
Difference in R. A.: Star preceding	0 1 35·272,
Difference in Declination: Comet south of Star	° ′ ″ 0 6 20·625.

The nucleus was fainter and smaller, and the Comet was evidently receding rapidly. Great care had to be taken in illuminating the webs, as it was so faint. The measurements given are, however, reliable, the greatest possible care having been taken. The micrometer was a Ramsden's, by Ross, of London.

Feb. 10.—The nucleus was again seen, but my professional engagements had detained me, and before any measurements could be taken it became overcast. The approximate position given by the Circles was*—

R. A. $23^h\ 55^m\ 27^s$, Dec. S. $33^\circ\ 28'\ 0''$.

Feb. 11, 12, 13.—Overcast.

Feb. 14.—Good measurements were obtained from σ *Sculptoris*.

The approximate place of the Comet by the Circles was—

R. A. $1^h\ 2^m\ 58^s$, Dec. S. $32^\circ\ 3'\ 0''$.

The differences by means of *seven* measurements at a mean of time $9^h\ 31^m\ 15^s$ Greenwich mean time were—

Difference in R. A.: Star preceding	$+0^h\ 5^m\ 1^s\cdot16$,
Difference in Declination: Comet south of Star	$+0^\circ\ 6'\ 35''\cdot85$.

The night was beautifully clear, and these were capital observations. I applied the spectroscope, but its light was too faint with my aperture to get any reliable reaction.

Feb. 15.—Splendid measurements were obtained from two stars nearly same R. A. and about 14′ apart, the Comet showing between them. [Sketch annexed.] Approximate position of Comet was—

R. A. $1^h\ 12^m\ 14^s\cdot35$, Decl. S. $31^\circ\ 33'\ 0''$.

The southernmost star I will call A, the northern B. They were about 5 and 6 mag., as far as I could estimate, but having no larger star catalogue than that contained in "Loomis," I am not able to identify them. At first I thought they were τ and π *Sculptoris*, but the distance between them is not sufficient. I have C. Dieu and Flammarion's maps, and there are marked stars corresponding to these in R. A. but in Decl. S. 34° 0′ 0″. I know my telescope is within a minute, and is so correct that I can find stars in that region easily from catalogue places without altering the setting of the verniers. There is no error in the sixth correction.

* On this day I fancied the Comet was affected by the attraction of *Jupiter*.

Means of Times =		h m s 9 20 11 G. M. T,
Difference in R. A. Comet preceding,	from A,	−0 2 5·2,
	from B,	−0 2 17·6,
Difference in Decl.	from A, Star north,	° ′ ″ 0 6 35·719,
	from B, Star south,	0 7 26·25.

These were also beautiful observations.

Feb. 16.—Again I had beautiful measurements. [Sket annexed.] There is a group similar to this in Flammario: map, but in different Declination to my reading, but there is a another group marked in correct position very similar. T above is a faithful sketch of the appearance at $10^h\ 16^m$ Greenwich mean time. The approximate position of the Con by the Circles was—

R. A.	h m s 1 23 55,
Dec. S.	° ′ ″ 30 57 0,
Difference in R. A.: Star preceding,	h m s + 0 3 8·264
Difference in Declination: Comet south of Star,	° ′ ″ + 0 5 5·288.

Feb. 17.—The nucleus was visible after a long search, b was too faint to bear illumination, so that measurements cou not be obtained. The approximate position by the Circles was

R. A. h m s 1 41 22·5 Decl. S. ° ′ ″ 30 2 0

After this "he went his way and I saw him no more," and much fear me I never shall again.

Latitude S. 37° 4′ 10″·97. Longitude E. $9^h\ 36^m\ 56^s$.

Castlemaine, Victoria, Australia,
1880, *October* 10.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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J. R. HIND, Esq., F.R.S., President, in the Chair.

Egerton W. Beck, Esq., 5 Blomfield Road, Shepherd's Bush;
Mons. R. L. Bischoffsheim, 3 Rue Taitbout, Paris;
Mons. Adolphe de Boë, Antwerp;
John Coles, Esq., Royal Geographical Society, 1 Savile Row, W.;
James Cook, Esq., Crow Hill, Oxford Street, Preston;
William Ernest Cooper, Esq., The Mount, near Worcester;
Staff-Commander W. B. Goldsmith, R.N., F.R.G.S, Royal Yacht "Elfin," Portsmouth;
Elijah Howarth, Esq., Public Museum, Sheffield;
Sydney T. Klein, Esq., 24 Belsize Park, N.W., and The Rock, Reigate Hill, Surrey;
Dr. Nicolas de Konkoly, Buda Pest, Hungary; and
The Rev. Joseph Reed, M.A., H.M.S. "Triumph;"

were balloted for and duly elected Fellows of the Society.

On the Bearing of Ancient Eclipses, and especially that of Agathocles, on the Moon's Motion. By Signor J. Celoria.

(*Extract from a letter addressed to the President.*)

J'ai l'honneur de vous envoyer un Mémoire sur quelques éclipses solaires anciennes, et sur celle d'Agathocle en particulier. Dans le même je commence pour référer des documents anciens, tirés des ouvrages de Cléomède, de Théon et de Ptolomée. Ce sont des documents qui individuent avec la plus grande évidence une

éclipse du Soleil qui a été : 1° totale sur l'Hellespont ; 2° visible et observée à Alexandrie ; 3° utilisée par Hipparque dans ses calculs ; la date seule y reste obscure et ignorée.

L'incertitude de notre théorie lunaire, l'indétermination des documents qui regardent toutes les éclipses de Soleil anciennes, l'importance qu'aurait sans doute dans la théorie de la Lune une éclipse solaire ancienne bien individuée, m'ont persuadé à faire des recherches sur la date de l'éclipse à laquelle se rapportent les documents indiqués.

J'ai par conséquence calculé toutes les éclipses totales de Soleil dont les dates arrivent entre le 1 janvier de l'année 331 et le 31 décembre de l'année 101 A. C. ; ou, selon la méthode astronomique de numéroter les années, entre le commencement de l'année −330, et la fin de l'année −100.

Parmi les éclipses calculées il n'y en a pas une seule dont, après les Tables de la lune de M. Hansen, la zone de totalité soit passée sur l'Hellespont ; on peut par conséquence conclure qu'aussi dans les siècles qui précèdent notre ère les Tables lunaires de M. Hansen ne représentent pas suffisamment bien les faits observés, et qu'aussi pour les époques anciennes il faut aux longitudes tabulaires de M. Hansen apporter une correction sensible. C'est une conséquence qu'on pouvait déjà présupposer, et que maintenant on peut dire démontrée rigoureusement ; à la conséquence même sont aussi favorables les calculs exécutés sur les éclipses du 14 mars de l'année −189, et du 29 août de l'année −115.

Des calculs exécutés et de la discussion faite dans le chapitre iv du Mémoire découle une seconde conséquence remarquable, c'est à dire que parmi les éclipses anciennes l'éclipse d'Agathocle est celle qui peut mieux satisfaire aux faits démontrés et affirmés par les documents indiqués, et que dans l'éclipse d'Agathocle la zone de totalité passa sur l'Hellespont.

Cela démontré, je développe une relation différentielle entre le déplacement $d\mathrm{D}$ de la zone de totalité pris sur la perpendiculaire à la direction de la ligne centrale, la correction dv de la longitude moyenne de la Lune, et la correction $d\Omega$ de la longitude du nœud. Le signe du déplacement $d\mathrm{D}$ est positif lorsque la zone de totalité doit être transportée vers le nord.

J'applique cette formule aux éclipses du 3 juin 1239, et du 6 octobre 1241, jadis étudiées par moi, et je trouve les deux relations—

$$+35'{\cdot}4 = -\ 1{\cdot}6786\, dv - 5{\cdot}3743\, d\Omega,$$
$$-42'{\cdot}6 = +30{\cdot}5820\, dv - 7{\cdot}1803\, d\Omega,$$

dans lesquelles les quantités $+35'{\cdot}4$ et $-42'{\cdot}6$ ne sont pas absolues, mais susceptibles d'augmentation ou de diminution, puisqu'elles dépendent de l'interprétation d'anciens documents ; leurs variations pourtant ne pourraient être trop grandes, et les équations écrites ne peuvent par conséquence être satisfaites que par des valeurs négatives de dv, ce qui s'accorde avec les recherches

de M. Newcomb, desquelles résulte aussi que les longitudes moyennes tabulaires de M. Hansen demandent pour des époques analogues à celles des éclipses de 1239 et de 1241 des corrections négatives.

L'éclipse d'Agathocle donne entre les quantités considérées la relation suivante :

$$dD = -5{\cdot}42633\,dv - 5{\cdot}42542\,d\Omega.$$

Dans cette relation la quantité dD doit être positive ; on voit par conséquence que pour la satisfaire, lorsqu'on ne veut admettre pour $d\Omega$ des valeurs trop grandes et incompatibles avec l'état actuel de notre théorie lunaire, il faut nécessairement prendre pour dv une valeur négative. Cette conséquence maintenant démontrée me paraît d'une importance capitale : non seulement dans les siècles postérieurs à notre ère, mais aussi dans les siècles antérieurs à l'ère même, les longitudes moyennes tabulaires de M. Hansen demandent une correction négative.

De la simple relation entre dD, dv et $d\Omega$, démontrée pour l'éclipse d'Agathocle, on ne peut naturellement rien déduire de certain sur les valeurs numériques de dv et $d\Omega$, mais il sera pourtant possible de voir si les hypothèses qu'on peut maintenant faire sur les valeurs mêmes sont plus ou moins conciliables avec la relation démontrée.

Je prends pour le coefficient de l'accélération séculaire du mouvement moyen de la lune la valeur théorique $6''{\cdot}176$ au lieu de la valeur adoptée par M. Hansen, et je combine la valeur de dv qui correspond à cette hypothèse avec des valeurs différentes de dD. Si on la combine avec la valeur de dD égale à $+188'$, c'est à dire si on transporte la ligne centrale de l'éclipse sur la pointe méridionale de l'Hellespont, on trouve pour $d\Omega$ la valeur $+150''{\cdot}27$; si on la combine avec la valeur de dD égale à $+240'{\cdot}7$, ou si sur la pointe méridionale de l'Hellespont on transporte la limite sud de la zone de totalité, on trouve pour $d\Omega$ la valeur $-432''{\cdot}58$. Selon le premier résultat, le mouvement du nœud adopté par M. Hansen exigerait, dans chaque siècle postérieur à 1800 et à partir de 1800 même, une correction négative et égale en valeur absolue à $7''{\cdot}12$; selon le second résultat, le même mouvement du nœud exigerait au contraire une correction séculaire positive et égale à $20''{\cdot}50$. Mais, en adoptant pour dD la valeur $+188'$ et *à fortiori* l'autre $+240'$, on transporte la zone de totalité de l'éclipse tellement au nord que le passage d'Agathocle devient impossible au sud de la Sicile. Or, quoiqu'il y ait des arguments favorables au passage d'Agathocle au nord de la Sicile, il serait pourtant arbitraire d'affirmer avec certitude, après la seule narration de Diodore, qu'Agathocle a suivi l'une plutôt que l'autre route. Pour se tenir à la lettre des documents existants, et pour ne pas faire pour le moment des hypothèses arbitraires, il faut déplacer la zone de totalité de manière qu'elle recouvre l'Hellespont, et qu'elle passe dans le même temps sur les deux positions

probables d'Agathocle, quelle que soit la route qu'il a suivie. A cet effet il suffit de prendre pour dD la valeur $+153'$ (valeur qui, combinée avec la valeur de dv qui correspond à la valeur théorique du coefficient de l'accélération séculaire, donne pour $d\Omega$ la valeur $+537''{\cdot}28$) et pour la correction séculaire du mouvement du nœud adopté par M. Hansen une valeur négative et égale à $25''{\cdot}46$.

L'incertitude de cette dernière valeur n'est pas grande lorsqu'on ne veut pas abandonner l'hypothèse du passage d'Agathocle au sud de la Sicile, et lorsqu'on n'oublie pas la grandeur de l'éclipse observée à Alexandrie, grandeur qui s'oppose à un trop grand déplacement de la zone de totalité vers le nord. Si on fait

$$dD = +153' + x,$$

la valeur de la correction séculaire du mouvement du nœud est représentée par l'expression

$$-25''{\cdot}46 + 0''{\cdot}0087x,$$

dans laquelle x est une quantité qui ne peut pas même devenir égale à $+300''$ sans rendre impossible le passage d'Agathocle au sud de la Sicile.

Tels sont les résultats principaux contenus dans le Mémoire que j'ai l'honneur de vous adresser, c'est à dire :

(1) La zone de totalité dans l'éclipse d'Agathocle doit passer sur l'Hellespont ;

(2) Non-seulement dans les siècles postérieurs à notre ère, mais aussi dans ceux antérieurs à la même, les longitudes moyennes tabulaires de la lune demandent une sensible correction négative ;

(3) La valeur théorique $6''{\cdot}176$ du coefficient de l'accélération séculaire du mouvement moyen de la lune est conciliable avec l'éclipse d'Agathocle lorsqu'on admet, pour le mouvement du nœud lunaire adopté par M. Hansen, une correction égale à $-25''$ dans chaque siècle postérieur à 1800 et à partir de 1800 même ;

(4) La valeur théorique du coefficient de l'accélération séculaire du mouvement moyen de la lune est conciliable encore avec l'éclipse d'Agathocle, et avec une correction *positive* séculaire du mouvement du nœud lunaire comprise entre $0''$ et $20''$ lorsqu'on ne veut pas admettre le passage d'Agathocle au sud de la Sicile.

Milan,
1880, *Décembre*.

On Mr. Stone's Alterations of Bessel's Refractions.

By W. H. M. Christie, Esq.

In a paper on Mean Refractions in the *Monthly Notices* for April 1880, Mr. Stone refers to some results which I gave in a paper on the Systematic Errors of the Greenwich N.P.D.'s, read at the meeting in January 1880, but not published till the middle of December last. As there would have been obvious inconvenience in following Mr. Stone in a discussion of an unpublished paper, I have preferred to defer any remarks on Mr. Stone's criticisms till the publication of my paper in the recently issued volume of the *Memoirs*. This has been the cause of the delay in my reply to Mr. Stone.

As regards the Greenwich circumpolar observations 1857 to 1865, Mr. Stone appears to be much concerned at the discordances between his residuals and mine; but the most important difference really admits of very simple explanation:—Mr. Stone had not stated what corrections he had applied, and he has not taken note of the corrections which I have applied, as specified in my paper. To begin with, Mr. Stone has not even allowed for the difference in our adopted co-latitudes, the corrections for which would increase my residuals by 0″·20. Then there is the still more important correction to the refractions beyond Z.D. 82°, which was applied to the Greenwich observations from the year 1857. Mr. Stone, it now appears, has brought back the results to Bessel's Refractions unaltered, whilst I have used the printed observations as affected with Main's correction. I certainly derived the impression from a perusal of Mr. Stone's paper of 1867, that he had taken no notice of this important correction, and I regret that there should have been this mistake on my part. Again, there is some doubt as to the exact limits of Mr. Stone's groups, for it so happens that as regards three of them, stars fall so near the boundary as to be easily transferred from one group to the next unless account is taken of the seconds of N.P.D. as well as of the degrees and minutes. In forming my residuals, as printed in the *Memoirs*, vol. xlv., I included θ *Ursæ Majoris* in the group 30° to 37° 40′, and f_1 *Cygni* in the group 37° 40′ to 43°. In both cases the precessional motion would carry the star, in the course of a few years, from one group to the next. Now, the transposition of the last-named star from the last group but one to the last group affects the last residual by 0″·47, which is more than twice the amount of Mr. Stone's estimate of the probable error of the result.

In order to give a fair comparison between Mr. Stone's residuals and my own, I have taken the results with the new law for R—D as given in my paper (*Memoirs*, vol. xlv. p. 165), and have applied a correction of −0″·20 to reduce to co-latitude 38° 31′ 21″·80,

and a correction to the last residual to remove the effect of Main's correction beyond Z.D. 82°, viz. $\frac{8}{12} \times -1''{\cdot}06 = -0''{\cdot}71$ when f_1 *Cygni* is included in the last group, and $\frac{8}{9} \times -1''{\cdot}06 = -0''{\cdot}94$ when that star is transferred to the preceding group. I have then applied the differences between Mr. Stone's residuals A and B to reduce to Mr. Stone's diminished refractions and co-latitude 38° 31′ 21″·61. The comparison of Mr. Stone's work and mine stands thus:—

N.P.D. above Pole − N.P.D. below,

with Mr. Stone's Refractions, Co-lat. 38° 31′ 21″·61, *and new Law for R − D.*

N. P. D.		0°–15°	15°–30°	30°–37° 40′	37° 40′–43°	43°–45½°
		″	″	″	″	″
Mr. Stone's values		−0·15	+0·10	+0·14	+0·08	−0·10
Mr. Christie's „	I.	−0·37	+0·02	+0·10	−0·25	−0·14
„ „	II.	−0·37	+0·02	+0·10	−0·18	−0·61

In the residuals I. f_1 *Cygni* is included in the last group; in II. it is transferred to the group 37° 40′–43°. These results of mine have been obtained by giving equal weight to each annual result, regardless of the number of observations. In the last group, four of the results have two observations S.P., and two or more above pole, and if double weight be given to these, the last residual of I. becomes − 0″·29.

Thus it appears that by simply applying the corrections for difference in adopted co-latitude and for Main's diminution in the refractions beyond Z.D. 82°, my last residual I. is brought into substantial agreement with Mr. Stone's, and, under these circumstances, I think I have some ground of complaint in regard to the language which Mr. Stone has used respecting the supposed discordance. His statement that either his results or mine are in error is based on a misapprehension, for which he is mainly responsible, in that he gave no explanation in his paper of 1867 of the corrections which he had applied to form his residuals and that he disregarded the explanation which I gave in presenting my residuals. I may add that I had no intention of casting any doubt on the accuracy of Mr. Stone's arithmetic, and that I did not start with any idea of examining his work. On discussing the circumpolar observations for the period 1851 to 1861 I arrived at results which gave so little support to Mr. Stone's diminution of Bessel's Refractions that I was naturally led to discuss the observations 1857 to 1865, which Mr. Stone had used, and finding that they did to a certain extent support his hypothesis, I thought it only fair to say so. But in giving a comparison between his results and mine, I stated that I did not know what corrections Mr. Stone had applied, and that the different system of weights which we had respectively used might perhaps account for a portion of the discordances. I

may remark that the difference in the R−D corrections with the old and new laws respectively affects the last residual by 0″·36, and that Mr. Stone had not given the slightest indication as to which formula he had adopted. If Mr. Stone had stated clearly in 1867 the corrections which he had applied to the Greenwich results, it might have been possible to verify his work. As it is, I must protest against the circumstance that discordances are still left between Mr. Stone's results and mine being taken as evidence of inaccuracy on either part. As regards my own work, I have, since the publication of Mr. Stone's paper, carefully recomputed the residuals, and have obtained identical results. The only changes I have made have been to transpose *β Ursæ Minoris* to the group 15°–30°,* *θ Ursæ Majoris* to the group 37° 40′–43°, and f_1 *Cygni* to the last group 43°–45½°.

I may add that I have not applied in this portion of my work corrections for error of the thermometer, as there is some uncertainty about the amount in individual years.

But, notwithstanding Mr. Stone's explicit statement in the *Monthly Notices* for April 1880, pp. 338, 339, of the corrections which he has applied in forming his last residual, there are several points to be cleared up:—

1. Corrections for errors of thermometer and barometer are stated to have been applied to the observations in 1857 and part of 1858. Now, I do not find in the Greenwich "Introductions" any observations in those two years which would fall in this group.

2. The limits of Mr. Stone's last group are N.P.D. 43° to 46° 29′. I have not been able to find any observations in the Greenwich "Introductions" between N.P.D. 45½° and 47½°.

3. The assumed co-latitude is stated to be 38°31′ 21″·80, but in his paper of 1867 Mr. Stone, after applying a correction −0″·205 obtains 38° 31′ 21″·61, which would imply an assumed co-latitude 38°31′21″·815. The difference is trifling, but it must be remembered that Mr. Stone gives his residuals to a thousandth of a second.

I call attention to (1) and (2), not in any spirit of criticism, but because there seems a possibility that as Mr. Stone extracted his results from the Greenwich Annual Catalogues, his last group may contain observations accidentally omitted from the Latitude Investigations in the Greenwich "Introductions" which I used.†

* In discussing the observations 1851–61 and 1862–65 I had taken 15½° as the limit of one of my groups, and had at first used the same limit for 1857–65. The correction only amounts to 0″·02.

† On examining the *Greenwich Observations* for 1857, I find that there are, as a matter of fact, several stars in the Annual Catalogue which, for some reason, are not included in the Latitude Investigation in the Introduction. Mr. Stone had not stated in his paper of 1867 that he had taken his results from the Annual Catalogues, and I thought it more natural that he should have taken them from the Introductions where they are collected. Thus our data are not the same.

On the rejection of the reflection observations by Mr. Stone I wish to offer a few remarks, not on account of its effect on the matter at issue, which is probably insignificant, but because it seems to be opposed to a principle usually accepted by astronomers—viz., that where an instrumental correction is determined by observations of a number of stars, the observation of each star may be used as a determination of its place, notwithstanding the fact that the observation in question has contributed *in a small degree* towards the determination of the instrumental correction on which the determination of place depends. Thus when the clock-error is deduced from transits of a number of clock-stars, the transit of each clock-star is used in connection with the clock-error to determine the R.A. of the star. Now, during the period 1857–1865 the zenith points were determined from observations extending over a fortnight, and including usually more than a dozen stars observed by reflection in each group. Thus the N.P.D. from the reflection observation of any one star would be virtually independent of that from the direct observation, since this star had only contributed in a small degree towards the determination of the zenith point. Again, for the R—D correction, two constants are determined by observations of a large number of stars, and the argument applies, therefore, with even greater force.

But, as regards the residuals, there is a point of some importance on which I am at direct issue with Mr. Stone—viz., the estimated probable error of the result for the last group. Mr. Stone obtains a weight of 28 or 24 with a probable error $0''\cdot2$, while with the same system of weights, I obtain a weight 6·5, with a probable error $0''\cdot4$. I have for the last group taken the probable error of an observation above the pole as $0''\cdot5$ and S.P. as $1''\cdot5$, and I have altogether 30 observations above pole and 17 below, which gives as an *inferior* limit to the probable error $0''\cdot38$, on the supposition that they all referred to the same star. It is just possible that the additional observations included by Mr. Stone may account for the discrepancy.

In any case there are, I believe, but few stars observed in this group; and where the same star—*Capella*, for instance—is observed every year on the same division of the circle, account ought to be taken of the probability of a systematic error * affecting the series. Thus the weight ought not to be assumed to be the sum of the weights for the separate years. Unless Mr. Stone has followed this last and erroneous system, I hardly see how he can have got such a large weight and small probable error for the result of his group.

I now pass on to the general question of Mr. Stone's proposed alterations in Bessel's Refractions.

* Such as division error or error of the micrometer screws. I have show later on that this last error would affect the residual for *Capella* in the gro 1868–1876 by $0''\cdot18$. And there may be unknown sources of error wh would have a still larger effect.

First, as regards the value of the constant of refraction :—

I gather from Mr. Stone's paper on the whole that he gives up the diminution of the mean refractions which he advocated in 1867 ; but he accompanies this concession by criticisms directed against the validity of the evidence in favour of Bessel's constant of refraction.

In the first place, he brings forward his discussion of the observations 1857–1865, and appeals to his residuals, and to the accumulated evidence of the Greenwich circumpolar observations in favour of his diminution of the mean refractions; while he calls attention to the very slight evidence on which Bessel's Refractions have been reintroduced. Now, it seems to me that Mr. Stone's alteration in the refractions was based on very inadequate data, and I cannot see any reason for his confining his attention to this particular period of nine years from 1857 to 1865. In his recent paper he has attempted to supplement it by a discussion of the period 1868–1876; but even then he only takes 18 years out of 42, and the last 9 years on which he relies do not lend much support to his views. So that it is really quite misleading to speak of the accumulated evidence of the Greenwich observations, when no account is taken of the 21 years of observation from 1836 to 1856 which tell against Mr. Stone's hypothesis. In the four periods, 1840–1847, 1851–1861, 1862–1865, and 1868–1876, which I have discussed, the first two support Bessel's Refractions, whilst the third and fourth show such large errors on either hypothesis that no certain conclusion can be drawn from them. The diagrams which I have given (*Memoirs*, vol. xlv. p. 161) show this at a glance much better than mere figures.

But Mr. Stone's hypothesis is undoubtedly supported by the circumpolar observations 1857–1865. His residuals for 1857–1865, with the diminished refractions, are :—

$$-0''.15 \qquad +0''.10 \qquad +0''.14 \qquad +0''.08 \qquad -0''.10;$$

or as I find them, with the law $b \sin z \cos^2 z$ for R—D,

$$-0''.37 \qquad +0''.02 \qquad +0''.10 \qquad -0''.25 \qquad -0''.14;$$

and with the law $b \sin z$,

$$-0''.33 \qquad +0''.20 \qquad +0''.37 \qquad +0''.07 \qquad +0''.22.$$

Against these we have to set the residuals, with Bessel's Refractions unaltered, for 1851–1861 :—

$$-0''.10 \quad +0''.02 \quad +0''.04 \quad +0''.27 \qquad +0''.15 \qquad +0''.04 \quad +0''.02 \quad -0''.02;$$

and for 1840–1847,

$$+0''.15 \quad +0''.05 \quad -0''.07 \quad -0''.18 \quad -0''.07 \quad +0''.02 \quad +0''.37 \quad +0''.20,$$

these last being taken in connection with Mr. Main's discussion of the observations below Z.D. 70° (*Memoirs*, vol. xxvi.).

These residuals compare favourably with Mr. Stone's, so that we have 19 years in favour of Bessel's Refractions, as against 9 years only in favour of Mr. Stone's alteration. And it is to be remarked that Mr. Stone has only formed five groups, thus simplifying the problem of obtaining small residuals. For with two quantities—viz., the co-latitude and the constant of refraction—at our disposal it is much easier to satisfy five equations than eight. But the mere circumstance that the residuals are small gives no indication by itself that the solution is deserving of confidence. In illustration of this, let us take the residuals from the Melbourne circumpolar observations 1863–1865, discussed by Mr. Stone in his paper of 1867. From these Mr. Stone deduces a correction of $+0''.15$ to the co-latitude, and of $-0.00372 \times$ refraction to Bessel's Refractions, and he adopts these without hesitation. But, after all, it appears that Bessel's Refraction undiminished, with Main's correction below Z.D. 82°, gives smaller residuals.

Mr. Stone's residuals for (S.P.D. above Pole — S.P.D. below) are:—

N.P.D.	180°–170°	170°–160°	160°–155°	155°–150°	150°–147°
	$-0''.23$	$+0''.20$	$+0''.44$	$-0''.03$	$-0''.39$;

while Bessel's Refractions, with correction $+0''.50$ to co-latitude, give:—

	-0.01	$+0.26$	$+0.27$	-0.59	-0.38
Weight	122	26	8	8	11.

Multiplying the two sets of residuals by $\sqrt{(\text{Weight})}$, so that each has a probable error corresponding to weight, we have them in a form more fitted for comparison:—

Mr. Stone's Refractions	$-2''.54$	$+1''.02$	$+1''.25$	$-0''.08$	$-1''.29$;
Bessel's ,,	-0.11	$+1.33$	$+0.76$	-1.67	-1.26.

Thus, on the whole, Bessel's Refractions give more accord residuals, the greatest error corresponding to the algebraic of 1 observation above and 1 below pole, being $1''.67$ Bessel's Refractions, and $2''.54$ with Mr. Stone's solution. is also seen on applying the usual criterion of least squares the sums of $(\text{residual})^2 \times$ weight are $10''.72$ with Mr. Sto

solution and only 6″·72 with mine. And yet *per se* Mr. Stone's residuals seem to leave little to be desired. If Mr. Stone had in this and in other cases given the probable error of his determination of x and y, the untrustworthiness of the solution would have been evident.

Somewhat similar criticism applies to the treatment adopted for the Greenwich circumpolar observations 1868–1876. The bulk of the observations are, as in the Melbourne results, included in the first two groups with vastly preponderating weight, and the correction to the refractions is sought to be established by means of two groups with small weights. The last group, 43°–46½°, formed by Mr. Stone contains 11 stars with total weight 34, exactly equivalent as regards number of stars and weight to each one of the 36 groups of 11 stars given in the Introduction to the Nine-Year Catalogue, p. 13. Of these 36 groups 23 are lumped together by Mr. Stone in a single group with weight 930, while the "all-important" last group has, as just mentioned, a weight 34 only. The degree of accuracy to be expected in a residual with this weight may be seen by examining the means of groups with corresponding or greater weight in the Introduction to the Nine-Year Catalogue, or the dots which represent these means in the diagram given in my paper in the *Memoirs*. If dots corresponding to Mr. Stone's last two groups be inserted on the diagram the difficulty of determining any correction to the co-latitude or to the refractions from the 38 dots here laid down will, I think, be manifest. But as Mr. Stone has grouped the residuals, the accidental errors are, to a great extent, masked, and the omission of the weights leaves us without any indication of the relative values of the several groups. Thus, presenting only five groups, he, by means of an arbitrary correction to the co-latitude, obtains small residuals, to which he appeals in proof of the soundness of his hypothesis. The only satisfactory way to test this, however, is to compare it with the results given with Bessel's Refractions, applying, as I have done throughout, Main's correction below Z.D. 82°.

Thus we have, taking ten groups corresponding to those in my paper on the Greenwich N.P.D.'s. :—

N.P.D. above Pole—N.P.D. below, 1868–1876.

With Mr. Stone's diminished Refractions and Co-latitude 38° 31′ 21″·60.

$-0''.21$ $+0''.18$ $+0''.28$ $+0''.76$ $+0''.28$ $+0''.24$ $+0''.28$ $+0''.52$ $+0''.72$ $+0''.19$;

With Bessel's Refractions and Co-latitude 38° 31′ 21″·90,

$-0''.10$ $+0''.28$ $+0''.35$ $+0''.75$ $+0''.12$ $-0''.02$ $-0''.07$ $-0''.08$ $-0''.34$ $-0''.61$.

In the latter part of his paper (*Monthly Notices*, vol. xl. p. 346), Mr. Stone gives for the group N.P.D. 41°-46½° with the diminished refractions and co-latitude 38° 31′ 21″·60, the residual +0″·71. With Bessel's Refractions and the same co-latitude, he gets −1″·33. To this must be applied a correction of +0″·60 to reduce to co-latitude 38° 31′ 21″·90, and a further correction of +0″·60 on account of Main's correction to refractions below Z.D. 82°. Thus with Bessel's Refractions as corrected by Main, we have the small residual −0″·13 to set against Mr. Stone's residual +0″·71.

In this case the observations to which Main's correction is applicable have a total weight of about ⅔ of that for the whole group. I have taken the mean value of Main's correction to this group as—1″·06 corresponding to mean N.P.D. 45° S.P. Thus for the three groups at large Z.D., Mr. Stone's diminution of Bessel's Mean Refractions gives somewhat larger residuals. Mr. Stone, of course, reduces his residuals by an appropriate correction to the co-latitude; but I would submit that this is not permissible. The annual results of the Greenwich observations show that the latitude has not changed in the course of more than 40 years, as Mr. Lewis Boss has pointed out in his memoir on the "Declinations of Stars" (p. 37),* and as may be seen from the results for co-latitude which I have collected in my paper (*Memoirs*, vol. xlv. pp. 156 and 167). So that we are not at liberty to assume one value for the observations 1857–1865, and another value for the observations 1868–1876.

As regards the observations of the Sun, I have fully discussed them for the whole period of 44 years, and I need not, therefore, further refer to the partial results which Mr. Stone adduces.

I now come to the comparison of the Greenwich observations with those at the Cape and Melbourne.

With respect to Melbourne, Mr. Stone, I think, does well point out the difficulty as regards the large correction to the r fractions north of the zenith, which he introduced (as he e presses it) "to force a close agreement between the resu But the influence of these on the determination of the refrac at Greenwich would have been clearer if he had given als comparison with the Nine-Year Catalogue uncorrected. comparison I now give, and it will readily be seen how fi are arguing in a circle :—

* I regret that this important work did not reach me till after was read, so that I could not make reference to it as I might othe done.

(*Corrected for Error of Thermometer at Greenwich.*)

N.P.D.	$G'_e - M$.	$G_e - M$.	$G'_e - M'$.	$G_e - M'$.
°	"	"	"	"
48	−0·37	−0·92	+2·33	+1·78
52	−0·25	−0·82	+1·67	+1·10
56	−0·04	−0·63	+1·41*	+0·82
60	+0·21	−0·41	+1·36	+0·74
64	+0·49	−0·16	+1·42	+0·77
68	+0·69	+0·01	+1·45	+0·77
72	+0·74	+0·03	+1·37	+0·66
76	+0·58	−0·16	+1·11	+0·37
80	+0·28	−0·50	+0·72	−0·06
84	+0·04	−0·78	+0·40	−0·42
88	+0·18	−0·69	+0·47	−0·40
92	+0·18	−0·74	+0·42	−0·50
96	+0·27	−0·71	+0·45	−0·53
100	+0·35	−0·72	+0·48	−0·59
104	+0·41	−0·77	+0·50	−0·68
108	+0·32	−0·99	+0·36	−0·95
112	+0·36	−1·16	+0·36	−1·16
116	+0·35	−1·51	+0·31	−1·55
120	+0·31	−2·15	+0·23	−2·23
122	+0·07	−1·84	−0·02	−1·93
124	+1·15	+0·33	+1·04	+0·22

G_e = Greenwich Nine-Year Catalogue with Bessel's Refractions (1 − ·00532).
G'_e = " " " with Bessel's Refractions.
M = Melbourne Catalogue 1870, with Stone's Corrections.
M′ = " " without " "

As Mr. Stone thinks he applied, in his paper of 1867, corrections for error of the thermometer in forming his residuals, though we have no direct evidence on the point, I have corrected the results for error of the thermometer at Greenwich (+0°·55) so as to put the case as favourably as possible for Mr. Stone. In my former paper I had not applied any such corrections to the Greenwich Catalogues, as there is some uncertainty with

* Mr. Stone gives this residual as 1″·65, which is apparently a misprint for 1″·40.

respect to the error of the thermometer in the case of three of them, and I should be inclined to say that Bessel's Refractions satisfy the Greenwich observations as they stand—*i.e.*, with an error of perhaps +0°·4 or +0°·5 in the thermometer.

I think it is clear that, whether we take the Melbourne N.P.D.'s, with or without Mr. Stone's corrections, the substitution of Bessel's Refractions for Mr. Stone's in the Greenwich Nine Year Catalogue greatly improves the residuals. And it is easy to distinguish between the effect of an alteration in the Greenwich refractions and the effect of an alteration in those at Melbourne. The former affects mainly the residuals at large N.P.D.'s, the latter those at small N.P.D.'s.

There can be no doubt that there is some systematic error in the Melbourne observations, increasing rapidly towards the north horizon,* and this must diminish our confidence in a comparison with this Catalogue. But it seems to me more probable that the discordances are due to some instrumental error than to a difference in the refractions north and south of the zenith. In the absence of detailed information on the flexure, R—D discordance, zenith points, and errors of the micrometer screws, it would be unsafe to suggest the cause or causes of systematic errors. It may be remembered that Professor Adams found a discordance amounting to 1″ in the results for nadir point of the Cambridge Transit Circle, according as the instrument had been pointing to the North or the South, and this was ultimately traced to a defect in the soldering of one of the ends to the central cube. This may suggest that we ought to take account of the possibility of discontinuous as well as continuous functions in the expression of instrumental errors, and that a discordance in the nadir point does not necessarily imply a correction of the form $a \cos z$ in the flexure. At Greenwich such a discordance was found to result from error in the micrometer screws, and to be peculiar to the nadir point observation itself. Thus we are not absolutely driven to the improbable supposition of large differences in the refractive power of the atmosphere at different places and on different sides of the zenith. The principle of forcing an agreement between observations by arbitrary alterations of latitude and refractions without much regard to physical facts does not seem very philosophical. By such a process we temporarily get over a difficulty, but do not get any nearer to the truth.

I am quite ready to admit that the comparison with the Melbourne observations alone would not be conclusive, but the Cape results seem to leave no doubt as to the substantial accuracy of Bessel's constant of refraction, as the following comparison shows:—

* N.P.D. 48° corresponds to Z.D. 80° N. at Melbourne and to Z.D. 76° N. at the Cape.

Greenwich Nine-Year Catalogue—Cape Catalogue (1860).

(*Corrected for Error of Thermometer at Greenwich.*)

N. P. D.	Bessel's Refractions.	Bessel's Refractions (1 − ·00532).
°	″	″
48	+0·88	−1·46
52	+0·80	−1·28
56	+0·66	−1·26
60	+0·56	−1·27
64	+0·54	−1·22
68	+0·59	−1·12
72	+0·62	−1·06
76	+0·52	−1·14
80	+0·31	−1·35
84	+0·08	−1·58
88	+0·01	−1·66
92	+0·04	−1·65
96	+0·10	−1·62
100	+0·13	−1·66
104	+0·14	−1·73
108	+0·06	−1·92
112	+0·06	−2·10
116	−0·08	−2·56
120	−0·15	−3·21
122	−0·35	−2·85
124	+0·78	−0·62

In the second set of residuals the effect of using the diminished refractions at the Cape as well as at Greenwich is shown. A correction of −0″·58 has been applied to the co-latitude of the Cape Observatory (56° 3′ 56″·44) as representing the effect of the diminution in the refractions.

Thus in favour of Bessel's constant of refraction we have the circumpolar observations for nineteen years, the comparisons with the Cape and Melbourne, and the observations of the Sun for twenty-eight years; and in favour of Mr. Stone's diminution of the constant, circumpolar observations for nine years only, and observations of the Sun for fifteen years. And even for the limited period on which Mr. Stone relies, the residuals are largely dependent on the assumption of the law $b \sin z \cos^2 z$ for R–D. I have taken no account of the circumpolar observations in the periods 1862–1865 and 1868–1876, as their evidence is inconclusive.

Secondly, with respect to the law of refraction:—It may be taken, I think, that Bessel's formula for the decrease of temperature in the atmosphere is not consistent with fact, and that this is the reason why his refractions for zenith distances beyond 80° do not accord with observation. This being so, I cannot see that anything will be gained by tinkering up Bessel's theory and distributing the errors in a different manner. Mr. Stone may easily get a tolerable agreement for the limited range of observations which he discusses—viz., 80° to 85° Z.D.; but his alteration would not satisfy a more extended series. Bessel's Refractions carry us very well as far as Z.D. 80°, and even perhaps to Z.D. 82°. Mr. Stone attempts to extend the range to Z.D. 85°, introducing, however, sensible discordances above Z.D. 80°. Is anything really gained by this? And how will observations below Z.D. 85° be satisfied? Bessel professed to satisfy the observed refraction at the horizon, and also secured a good agreement with observation as far as Z.D. 80°; Mr. Stone throws over all refractions below Z.D. 85°, and introduces divergences from other theories as well as from observation between 75° and 80°. I am prepared to admit that Bessel's is an artificial theory, and that his law of refraction is erroneous, though the constant satisfies observations; but there is a great convenience in retaining refractions which have been so generally used, until we have tables founded on a really satisfactory theory with which to replace them. Uniformity of system will greatly facilitate the determination of corrections. I have called attention in my former paper to the circumstance that Bessel's rate of decrease of temperature in the atmosphere is too small at small altitudes and too large at great altitudes. Laplace's hypothesis agrees much better with observations of temperature made during balloon ascents, and it makes the refraction at 85° smaller than Bessel's by 1″·93, which is fully two-thirds of the correction indicated by observation.

I do not propose to enter further on this question now. My reason for alluding to it is to point out that in order to secure an agreement at 85° there is no occasion to introduce comparatively large corrections to Bessel between 75° and 80° Z.D. The differences (Bessel—Laplace)* are only −0″·09 at 75° Z.D., and −0″·17 at 80° Z.D.; while those for Bessel—Stone are +0″·14 and +0″·46. As the differences between Lubbock's Refractions and Bessel's are equally small, I think we may safely assume that at Z.D. 80° Bessel's theory is not in error by more than 0″·2, which only amounts to ·0006 × refraction, or about $\frac{1}{1600}$th part.

This point has an important bearing on the determination of

* Using Caillet's Tables (*Conn. des Temps*, 1851) calculated on Laplace's theory, and reducing to Bessel's constant of refraction and to temperature 50° F., I find the following differences (Bessel—Laplace):—0″·00 at Z.D. 45° and 60°; −0″·09 at 75°; −0″·17 at 80°; −0″·18 at 81°; −0″·12 at 82°; +0″·08 at 83°; +0″ 61 at 84°; +1″·02 at 84½°; +1″·93 at 85°.

the constant of refraction. For it is clear that if we want to find the correction y to this constant within such limits as ·003, an error of ·0006 in Bessel's theory is of no practical importance, and thus we extend our range of Z.D. to 80°, and double the effect on our residuals. Even Mr. Stone's discordance from Bessel's theory of +0″·46 would only imply an error of ·0015 × refraction; so that on his own showing there is no occasion to increase the difficulties of the problem by restricting the range to 75° Z.D.*

But the case is very different beyond 82° Z.D., where the discordances between different theories begin to increase rapidly in proportion to the refractions. It seems to me, then, that we may safely use Bessel's Refractions as far as Z.D. 80° for the determination of the constant of refraction, since the error of theory is but a small fraction of the uncertainty of the constant; and that beyond 82° Z.D., the conditions being reversed, observations may be used for determining the law of refraction, on the assumption that the constant is known with tolerable accuracy.

Thus for the determination of y from circumpolar stars, instead of Mr. Stone's limiting equations

$$x + 90y = a_l, \qquad x + 215y = a_m,$$

we have

$$x + 90y = a_l, \qquad x + 313y = a_n;$$

and from results at northern and southern Observatories, instead of

$$x + 116y = b_l, \qquad x + 230y = b_m,$$

we have

$$x + 116y = b_l, \qquad x + 316y = b_n;$$

and an error of ·003 in y will introduce into our residuals a_l and a_n a difference of about 0″·65 instead of 0″·35.

I have taken Mr. Stone's limiting equations merely for the sake of comparison between the limits 75° and 80° Z.D. They are a trifle too favourable to the circumpolar observations, and perhaps hardly fair to those at northern and southern Observatories. We ought, I think, to assume some such equations as these:—

$$x + 92y = a_l, \qquad x + 313y = a_n,$$
$$x + 75y = b_l, \qquad x + 316y = b_n,$$

* I must admit that on the second page of my paper (*Memoirs*, vol. xlv. p. 152) I have spoken of Z.D. 75° as the limit at which the law of refraction begins to get somewhat uncertain, but on the next page and subsequently I have taken 78½°, and finally I have concluded that Bessel's Refractions are sensibly correct as far as Z.D. 80°. In explanation of these apparent inconsistencies, I may remark that the various theories of refraction *begin* to diverge about Z.D. 75°, but the divergence in the case of Bessel's, Laplace's, and Lubbock's Refractions is of little practical importance till beyond Z.D. 80°.

the sum of refractions for *Polaris* above and below pole being 92″, and the refraction for a star at N.P.D. 81° equidistant from the zeniths of Greenwich and the Cape being 53″. I have taken the coefficient of y as $53\sqrt{2}$, assuming the probable errors of the determinations of y and z (the correction to the constant of refraction for the Cape) to be the same. I do not, however, insist on this point, as the actual problem consists in the determination of *three* unknown quantities from a system of equations with different weights, and seems hardly to admit of statement in the simple form of two limiting equations with only two unknown quantities.

The greatest difference of residuals from circumpolar observations would be $221y$, whilst from northern and southern Observatories we should have $241y$.

These equations give, according to my view, a fair comparison of the relative value of circumpolar observations and of results at northern and southern Observatories for the determination of y, on the supposition that we are at liberty to assume different values of y for Greenwich and the Cape—an hypothesis which I am not prepared to admit without further evidence.

But, as I have shown in my paper on the Systematic Errors of the Greenwich N.P.D.'s, the case is far more favourable when we assume y the same at the two Observatories. For the satisfactory comparison, however, of observations at Greenwich and southern Observatories, we require a thorough discussion of the latitude, flexure, R−D correction and errors of micrometer screws for the Cape and Melbourne.* Reflection observations of stars extending over a large range of Z.D. are particularly wanted at Greenwich, as well as at the southern Observatories, so that the R−D correction may be determined from a curve, and not from an arbitrary law. We may hope in due course to have these data, and then we shall be able to make use under the most favourable conditions of the comparisons of the results at northern and southern observatories.

In discussing the circumpolar observations for the group Z.D. 80° to 85°, 1868–1876, Mr. Stone makes a remark (*Monthly Notices*, vol. xl. p. 345) which, I think, must have been written in momentary forgetfulness of the system which has been in use at Greenwich for upwards of 40 years, including the period during which he himself was there. I allude to the system of making the determinations virtually dependent on observations of stars alone. Mr. Stone says: "The co-latitude during the second period [1868–1876] appears to have been affected by the constant difference which existed during the period 1870–1874 between the nadir point determinations, and the virtual rejection of the observations with the reflecting eyepiece, by the application of constant corrections, to make their results agree in mean results

* In the *Monthly Notices* for 1879 December Mr. Stone has given the results of a discussion of the latitude of the Cape, which afford valuable information on this point, and we may hope for fuller details when the Cape Catalogue for 1880 is published.

with the zenith points found from the star observations. To some extent, however, any error which might arise from the course adopted will be compensated for when the co-latitude obtained from the observations themselves is adopted, as in the present comparison."

Now, it is well known that since 1836 the practice at Greenwich has been to deduce the R−D correction from observations of stars alone, and to include in it a constant term which represents the correction to adopted zenith points to make them agree, in mean results, with those found from the star observations. It is clear that it can make no difference in mean results for a year, whether we apply the correction at the end of the year to make the adopted zenith points agree with those resulting from stars, or whether we apply the requisite correction to each nadir point observation to make its results agree in the mean with those given by stars. The latter course was adopted in the years 1871–1874, and that it was virtually equivalent to the constant correction included in R−D in 1869 and 1870 is shown by the smallness of the constant term in R−D after the correction to the nadir point observation had been applied.

Mr. Stone's language would imply that the source of the discordance in the nadir point observation is unknown, and as its existence would then cast discredit on the results 1868–1876, it may be well to make a few remarks on it here, though I have already explained its origin in the errors of the micrometer screws in a former paper (*Monthly Notices*, vol. xxxvii. p. 18).

In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^r of the microscope micrometers, and thus was obtained that accordance of separate results which is so dear to some astronomers, but which is not always conducive to the elimination of systematic errors. The additional wear resulting from this increased use of one part of the screws gave rise to a discordance between the nadir observation and observations of stars which were taken at all parts of the screws. This discordance is shown in the years 1869 and 1870 by the large constant term in R−D; in subsequent years a correction was applied to each nadir observation derived from the mean results for the preceding year. In 1875, being dissatisfied with the large correction for runs virtually required to reduce the nadir observation to the mean of observations of stars which would fall at about $2^r{\cdot}5$, I suggested that the nadir observation should be taken at from $2^r{\cdot}5$ to $3^r{\cdot}5$, and it was then found that the correction to the nadir observation had changed from $-0''{\cdot}8$ to $+0''{\cdot}6$. This threw suspicion on the micrometer screws, and their errors were examined, as explained in my paper of 1876. The result was to show that corrections were required ranging from $-1''{\cdot}4$ at $1^r{\cdot}3$ to $+0''{\cdot}8$ at $2^r{\cdot}3$, the correction at $0^r{\cdot}0$ being $-0''{\cdot}9$, corresponding very closely with the correction to the nadir observation $-0''{\cdot}8$. Further, observations taken for another purpose in 1868 showed that the errors of the micrometer

screws were sensibly the same at that time, with the exception of the portion about $0^r{\cdot}0$, which had suffered extra wear since 1868 from a preponderance of some 4000 observations taken at this part. On inspecting the threads of the screws they were found to be much worn, as indicated by the errors. Thus there is no mystery about the discordance of the nadir observation, and there is no need to assume a term $a \cos z$ in the flexure. The apparent change in the co-latitude on which Mr. Stone remarks has nothing to do with this correction, but is simply due to a change in the system of weights given to the close circumpolars and to the increased number of observations of wide circumpolars. It did not begin in 1871, when the correction to the nadir observation was first applied, but in 1872, when special attention was directed to the observation of wide circumpolars. Previous to that the large weight given to the four close circumpolars made the value of the co-latitude virtually depend on them, and if this system had been continued the same value would have been obtained in subsequent years. The four close circumpolars give for the period 1868–1876 38° 31′ 21″·95 with Bessel's Refractions, and 38° 31′ 21″·70 with the diminished refractions; whereas the stars about N.P.D. 22° give about 38° 31′ 21″·50 and 38° 31′ 21″·17. It is simply the admission of these latter stars to a fair share in the result which has produced an apparent change. This apparent change does not justify Mr. Stone in using two different co-latitudes in the two periods 1857–1865 and 1868–1876.

Mr. Stone has done good work in collecting the results for north refraction stars; but before making use of them it is important to know whether he has applied the corrections for errors of the micrometer screws. In the case of *Capella*, which has the largest weight of any star in the group, and which has been observed in every year, I find that the correction applicable on this account to Mr. Stone's "Excess of N.P.D. below Pole" amounts to −0″·18, and the corrections for other stars may easily be larger. The necessity for similar corrections in the period 1857–1865 suggests the exercise of caution in accepting estimates of probable error based simply on the number of observations, even when the possibility of a small outstanding division error is included. The corrections to the residuals in 1874 for λ *Ursæ Minoris* (47 observations), *Polaris* (152 observations), *Cephei* 51 (60 observations), and δ *Ursæ Minoris* (57 observations), on account of error in the micrometer screws were −0″·11, +0″·25, −0″·52, and +0″·39 respectively. Corrections of such magnitude to the mean results of a large number of observations would hardly have been expected.

Blackheath,
1881, *January* 12.

Correction to Paper "On the Systematic Errors of the Greenwich North Polar Distances," *Memoirs*, vol. xlv. p. 156.

The Flexure correction for 1851 and 1852 in the table should be +0″·73 instead of +0″·50. The value adopted is not stated in the "Introductions" to the *Greenwich Observations*, and it was assumed to have been the same as for 1853; but on referring to the original records it appears that the value +0″·73 was used, being the mean of the determinations in December 1850 and February 1851, giving double weight to the latter.

Spectroscopic Results for the Motions of Stars in the Line of Sight, obtained at the Royal Observatory, Greenwich, IV.

(*Communicated by the Astronomer Royal.*)

The results for star motions here given are in continuation of those printed in the *Monthly Notices*, vol. xxxvi. p. 318, vol. xxxvii. p. 22, and vol. xxxviii. p. 493. The observations were made with the "half-prism" spectroscope, one "half-prism" giving a dispersion of about 18½° from A to H being used, except in a few cases of bright stars mentioned in the remarks, where a train of two "half-prisms," with a dispersion of 83° from A to H, was used. These dispersions have been inferred by measurement of the distance between b_1 and b_4 as compared with the wave-length measure. An eyepiece with a magnifying power of 14 was used. In some of the recent observations, since 1880 March, the pointer in the micrometer was coated with Balmain's luminous paint, forming a bright line with which the star-line and comparison line were respectively bisected. In other cases the field was illuminated by a luminous-painted diaphragm placed in the micrometer adapter. This phosphorescent illumination was found to be much superior to artificial light.

Motions of Stars in the Line of Sight in Miles per Second, observed with the Half-Prism Spectroscope.

(+ denotes Recession; − Approach.)

The initials W. C. and M. are those of Mr. Christie and Mr. Maunder respectively.

The weights are on a scale of ½ to 5 for each observation.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Andromedæ.			
1879, Sept. 3	M.	6	F	− 8·5	−13·6	−20·5	
1880, Nov. 8	M.	6	F	+ 9·5	−37·0	−35·4	Misty. Spectrum steady though faint.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				β Cassiopeiæ.			
1880, Oct. 1	M.	7	F	− 4·7	+32·4	+40·9	Spectrum steady though faint.
				γ Pegasi.			
1879, Sept. 3	M.	4	F	− 7·8	− 8·1	− 7·8	
1880, Nov. 17	W.C.	2	F	+13·8	−28·6	−20·7	
27	M.	12	F	+15·7	−61·6	−63·5	
				α Cassiopeiæ.			
1880, Feb. 21	M.	5	b_1	+11·5	− 0·4	+ 1·9	Interrupted by cloud.
Nov. 20	M.	4	b_1	+ 5·0	+37·1	+42·7	
				γ Cassiopeiæ.			
1880, Oct. 1	M.	9	F	− 6·6	−18·2	−23·0	Steady. Definition good.
Nov. 21	W.C.	3	F	+ 3·8	−55·3	−35·9	Definition bad.
				β Andromedæ.			
1878, Nov. 2	M.	9	b_1	+ 3·4	+ 9·6	+17·8	Definition good.
1880, Sept. 29	M.	9	b_1	− 6·0	+ 9·3	+ 8·9	Definition fair.
				β Arietis.			
1879, Sept. 3	M.	5	F	−17·7	−10·6	−12·5	Near the Moon. Faint.
1880, Nov. 17	W.C.	6	F	− 7·9	−20·1	−12·1	
				α Piscium.			
1880, Nov. 17	W.C.	1½	F	+ 8·7	−24·8	− 31·6	
27	M.	6	F	+11·5	−33·2	−43·4	Faint. Definition poor.
				γ¹ Andromedæ.			
1878, Nov. 2	M.	4	b_1	− 0·6	+ 3·9	+ 8·7	Definition good.
1880, Feb. 21	M.	6	b_1	+15·5	−34·4	−31·4	Definition fair.
				α Arietis.			
1878, Sept. 18	M.	3	b_1	−11·5	−18·1	−14·5	Definition poor.
Oct. 22	M.	4	b_1	− 2·1	−16·2	−22·1	Definition fair.
Nov. 2	M.	1	b_1	+ 1·4	− 1·7	− 1·4	Two-prism train.
1880, Nov. 20	M.	3	b_1	+ 7·3	−32·8	−33·8	

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Ceti.			
1878, Oct. 22	M.	2	b_1	− 4·1	−27·5	−30·4	Definition poor.
22	M.	2	b_4	− 4·1	−33·5	−43·1	b_4 not clearly isolated.
				β Persei.			
1880, Nov. 17	W.C.	3	F	+ 0·5	−24·1	−29·5	
				α Persei.			
1880, Nov. 8	M.	6	F	− 3·8	+26·2	+26·5	Misty. Steady but faint.
28	W.C.	1	F	+ 1·9	+11·7	+15·3	Sky hazy.
				ζ Persei.			
1880, Feb. 17	M.	1	b_1	+13·4	−40·4	−49·5	Star-lines faint.
				Aldebaran.			
1880, Jan. 26	M.	4	b_1	+16·0	+22·4	+22·9	Definition very poor.
				Capella.			
1880, Jan. 26	M.	6	b_1	+12·6	+17·7	+19·5	Definition poor.
				Rigel.			
1880, Jan. 30	M.	6	F	+13·3	+13·2	+ 7·3	Very tremulous. Direct comparison.
Mar. 13	M.	4	F	+15·7	+13·4	+12·8	
				γ Orionis.			
1880, Jan. 30	M.	8	F	+14·1	− 1·1	+ 0·3	
Mar. 17	M.	2	F	+17·5	− 5·7	− 7·1	Definition poor.
				β Tauri.			
1880, Jan. 30	M.	6	F	+14·3	− 8·7	− 9·7	Definition fair.
Mar. 15	M.	8	F	+18·5	−27·1	−29·1	Definition good.
				δ Orionis.			
1880, Jan. 30	M.	2	F	+13·2	− 2·0	− 0·8	Definition poor.
Mar. 13	M.	3	F	+17·0	−18·1	−15·7	Definition variable.
				ε Orionis.			
1880, Jan. 30	M.	6	F	+12·9	−37·6	−36·9	Definition fair.
Mar. 17	M.	4	F	+16·8	−31·9	−27·6	Spectrum tremulous.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				ζ Orionis.			
1880, Jan. 30	M.	3	F	+12·6	+ 7·3	+ 4·6	Definition poor.
Mar. 17	M.	2	F	+16·7	− 0·3	− 0·9	Definition poor.
				κ Oricnis.			
1880, Jan. 30	M.	2	F	+11·4	+ 4·2	+ 3·6	Spectrum very tremulous.
Mar. 13	M.	2	F	+15·6	− 6·8	− 5·3	Spectrum very tremulous.
				α Orionis.			
1880, Jan. 26	M.	4	b_1	+11·5	+24·6	+20·6	Definition poor.
				β Aurigæ.			
1880, Mar. 13	M.	2	F	+17·2	−11·1	− 9·5	Definition fair.
Apr. 8	M.	8	F	+15·9	−12·1	−11·6	Definition very good.
				γ Geminorum.			
1880, Feb. 10	M.	1	F	+13·0	+18·8	+23·8	Observation unsatisfactory.
Mar. 15	M.	8	F	+18 1	− 2·8	+ 1·3	Definition good. Steady.
				Sirius.			
1880, Jan. 30	M.	8	F	+ 6·9	+28·2	+30·0	Definition poor.
30	M.	12	F	+ 6·9	+27·0	+22·6	Two-prism train.
Mar. 19	M.	8	F	+13·9	+ 1·3	+ 5·8	Two-prism train.
24	M.	9	F	+14·1	+ 1·6	+ 6·1	Two-prism train.
				β Canis Minoris.			
1880, Mar. 17	M.	3	F	+16·6	−14·4	−14·6	
				Castor.			
1880, Mar. 24	M.	4	F	+17·6	0·0	− 1·0	Two-prism train.
				Procyon.			
1880, Mar. 19	M.	8	F	+16·2	+ 3·0	+ 8·0	Two-prism train.
24	M.	10	F	+16·7	+ 7·1	+ 6·0	Two-prism train.
				Pollux.			
1880, Jan.				+ 4·5	−39·5	−43·1	Definition poor.

Date.	Obscr-ver.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Hydræ.			
1880, Feb. 21	M.	3	b_1	+ 2·2	+35·6	+36·7	
				ε Leonis.			
1880, Feb. 17	M.	2	b_1	+ 3·1	− 5·1	− 3·1	
21	M.	3	b_1	+ 4·4	− 1·1	− 0·9	
				Regulus.			
1880, Mar. 17	M.	7	F	+ 9·1	+17·5	+15·6	Definition fair.
				γ¹ Leonis.			
1880, Feb. 17	M.	3	b_1	+10·3	−43·3	−40·2	Definition poor.
				β Ursæ Majoris.			
1879, May 21	M.	2	F	+12·3	+18·3	+ 5·2	Definition bad.
June 9	M.	5	F	+10·8	+49·2	+49·1	
1880, Apr. 8	M.	4	F	+11·4	0·0	+ 3·5	Definition fair.
May 22	M.	2	F	+12·2	+12·9	+11·7	Definition poor.
				α Ursæ Majoris.			
1880, Feb. 21	M.	2	b_1	+ 4·0	− 4·0	− 4·0	Definition bad. Very tremulous.
				δ Leonis.			
1880. Mar. 17	M.	8	F	+ 5·6	−38·4	−36·2	Definition good.
May 15	M.	6	F	+16·9	−15·1	−14·0	Definition fair.
				θ Leonis.			
1880, Mar. 17	M.	2	F	+ 5·0	+13·0	+ 9·3	Definition good.
				χ Ursæ Majoris.			
1879, May 21	M.	1	F	+13·4	+25·8	+43·6	Very tremulous. Doubtful.
				β Leonis.			
1880, Mar. 17	M.	4	F	+ 2·5	+10·2	+ 7·3	Definition poor.
				γ Ursæ Majoris.			
1879, May 21	M.	4	F	+12·2	+ 8·2	+ 6·3	Tremulous.
June 9	M.	6	F	+11·4	+23·5	+30·2	
1880. Apr. 8	M.	6	F	+ 9·7	+20·0	+18·4	
May 22	M.	2	F	+12·2	+ 4·3	+ 2·2	Definition poor. Faint.
June 28	M.	6	F	+ 9·4	+ 4·4	+11·8	Definition fair.

Date.	Observer.	Weight.	Lines.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Reduced.	Remarks.
				δ Ursæ Majoris.			
1880, June 28	M.	2	F	+ 8·6	+20·8	+29·5	
				γ Virginis.			
1880, Mar. 17	M.	4	F	− 3·5	+57·4	+51·0	Definition fair.
May 1	M.	2	F	+ 9·8	+12·6	+16·5	
15	M.	1	F	+13·0	+27·4	+45·4	Definition poor.
				ε Ursæ Majoris.			
1879, May 21	M.	5	F	+10·4	+24·4	+51·2	
June 9	M.	4	F	+10·2	+ 9·8	+14·8	
1880, May 22	M.	4	F	+10·4	+ 0·2	+ 0·4	Definition poor.
June 4	M.	8	F	+10·3	+15·4	+20·9	Definition good.
				δ Virginis.			
1880, Feb. 26	M.	1	b_4	− 9·7	+62·7	+51·8	Definition poor.
				α Canum Venaticorum.			
1880, May 25	M.	8	F	+13·0	−30·2	−29·6	Definition good.
				ε Virginis.			
1880, Feb. 21	M.	2	b_1	−10·4	−28·9	−32·0	
26	M.	2	b_1	− 9·1	−26·7	−25·2	Definition poor.
				Spica.			
1880, May 1	M.	6	F	+ 6·1	+ 0·3	+ 1·8	Tremulous. Definition poor.
15	M.	6	F	+ 9·9	− 5·4	− 4·1	Very tremulous.
25	M.	4	F	+12·2	− 2·2	− 2·9	Very tremulous.
				ζ Ursæ Majoris.			
1880, June 4	M.	6	F	+ 9·9	+ 7·2	+11·4	Definition fair.
28	M.	4	F	+ 9·0	+35·1	+49·3	
				η Ursæ Majoris.			
1880, June 4	M.	6	F	+10·2	+14·2	+25·2	Definition good.
				Arcturus.			
1880, May 29	M.	7	b_4	+11·1	−24·6	−27·2	Definition bad.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				γ Boötis.			
1880, July 7	W.C.	2	F	+11·6	−50·5	−53·2	
				*e*² *Boötis.*			
1880, Feb. 26	M.	4	b_1	−10·6	−14·3	− 8·3	Definition fair.
May 29	M.	2	b_1	+ 9·2	− 1·4	0·0	Definition bad.
				β Ursæ Minoris.			
1880, Feb. 21	M.	5	b_1	+ 2·0	+24·0	+19·7	Tremulous. Definition poor.
Sept. 13	M.	3	b_1	+ 3·9	+32·6	+31·6	Definition poor.
				β Libræ.			
1880, May 8	M.	12	F	+ 0·3	+ 7·4	+ 7·6	Definition good.
18	M.	4	F	+ 3·3	+12·4	+10·6	Tremulous. Definition poor.
July 13	W.C.	2	F	+15·8	+35·0	+31·0	Hazy. Spectrum tremulous.
				α Coronæ.			
1879, Sept. 3	M.	2	F	+11·2	+22·9	+22·0	
1880, May 18	M.	6	F	+ 3·9	+10·3	+ 8·5	Definition poor.
				α Serpentis.			
1880, Feb. 26	M.	3	b_4	−16·1	−30·5	−19·3	
				ε Serpentis.			
1880, May 8	M.	6	F	− 1·0	+44·5	+40·3	Definition good.
18	M.	4	F	+ 0·8	+25·8	+19·8	Definition poor.
				γ Herculis.			
1880, May 15	M.	1	F	− 0·5	−52·8	−82·3	Faint, but definition fair.
25	M.	1	F	+ 1·8	+ 7·8	+ 7·5	Definition bad. Very doubtful.
				η Draconis.			
1880, Sept. 13	M.	1	b_1	+ 1·3	+21·4	+20·4	Tremulous. Definition poor.
20	M.	4	b_1	+ 0·9	−27·2	−35·2	Definition fair.
29	M.	3½	b_1	− 0·3	+36·1	+39·2	Tremulous.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				β Herculis.			
1879, July 23	M.	1½	b_1	+11·5	+42·9	+41·6	Observation doubtful.
				ζ Herculis.			
1879, Aug. 22	M.	3	b_1	+10·7	+41·6	+30·2	Interrupted by cloud.
25	M.	4	b_1	+10·7	+18·0	+15·7	
				ζ Draconis.			
1882, Oct. 1	M.	2	F	+ 1·5	−23·7	−27·0	Faint but steady.
Nov. 8	M.	3	F	+ 1·2	−20·5	−18·6	Faint but steady.
				β Draconis.			
1880, Sept. 13	M.	2	b_1	+ 4·5	+40·5	+32·2	Tremulous. Definition poor.
27	M.	6	b_1	+ 4·2	+16·0	+18·7	Mist. Faint but steady.
				α Ophiuchi.			
880, July 15	M.	4	F	+ 7·9	−21·5	−19·6	Definition fair.
29	M.	6	F	+10·5	+14·9	+16·1	Definition fair.
Sept. 3	M.	1	F	+14·5	+25·7	+24·2	Definition bad.
				ξ Draconis.			
1880, Sept. 27	M.	1	b_1	+ 3·0	+ 8·3	+11·0	Observation doubtful.
				γ Draconis.			
1880, Sept. 13	M.	4	b_1	+ 4·7	−11·0	−11·4	Definition fair.
				α Lyræ.			
1880, July 29	M.	8	F	+ 3·4	−23·7	−25·2	Definition fair.
29	M.	8	F	+ 3·4	−15·8	−11·7	Two-prism train.
Sept. 3	M.	6	F	+ 7·3	−26·4	−28·2	Tremulous. Definition bad.
Nov. 27	M.	12	F	+ 5·5	−33·1	−40·0	
				γ Lyræ.			
1879, Sept. 3	M.	8	F	+ 8·0	+ 7·4	+ 6·7	Steady.
1880, Nov. 27	M.	6	F	+ 7·6	+ 8·6	+16·3	
				ζ Aquilæ.			
1880, July 15	M.	6	F	+ 1·4	−27·4	−23·8	Definition fair.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				δ Aquilæ.			
1880, July 15	M.	1	F	+ 0·5	+18·7	+17·4	Definition poor.
				β Cygni.			
1879, Aug. 25	M.	3	b_1	+ 6·4	−31·3	−38·5	Definition fair.
1880, Sept. 20	M.	3	b_1	+10·2	− 3·9	− 1·5	Interrupted by cloud and mist.
Nov. 20	M.	4	b_1	+10·8	−13·1	−22·3	
				γ Aquilæ.			
1879, Sept. 2	M.	1	b_1	+10·1	−49·3	−48·5	Mist. Definition poor.
				δ Cygni.			
1880, July 29	M.	2	F	− 1·0	−29·0	−27·6	
Sept. 3	M.	1	F	− 3·6	−22·8	−21·5	Faint. Definition poor.
Nov. 8	M.	10	F	− 8·1	− 6·0	− 6·6	Mist. Faint but steady.
				α Aquilæ.			
1880, July 20	M.	12	F	− 0·4	−10·5	−13·6	
				γ Cygni.			
1879, Sept. 3	M.	6	F	+ 3·0	+ 8·3	+ 5·3	
1880, Sept. 29	M.	4	b_1	− 6·9	− 5·2	− 6·9	Faint and tremulous.
				α Delphini.			
1879, Sept. 3	M.	2	F	+ 6·5	+21·3	+19·4	
1880, July 20	M.	1	F	− 4·4	+25·3	+24·3	Faint. Definition poor.
				α Cygni.			
1880, Sept. 29	M.	4	b_1	+ 5·1	−18·4	−20·5	Tremulous. Definition poor.
				ε Cygni.			
1878, Oct. 22	M.	5	b_1	+10·8	+14·8	+17·4	Tremulous.
				ζ Cygni.			
1880, Nov. 20	M.	1	b_1	−13·0	− 6·7	−18·8	Definition poor.

Date.	Observer.	Weight.	Line.	Earth's motion in miles per sec.	Concluded motion of star. Measured.	Estd.	Remarks.
				α Cephei.			
1880, Nov. 21	W.C.	2	F	+ 5·1	−75·9	−48·6	
				β² Cephei.			
1880, Nov. 20	M.	3	b_1	− 2·7	+53·1	+53·8	Spectrum faint.
				ε Pegasi.			
1878, Sept. 18	M.	2	b_4	+ 7·2	−15·7	−14·6	Definition poor.
Oct. 22	M.	5	b_1	+14·8	−20·8	−20·5	Definition fair.
25	W.C.	2	b_1	+15·2	−23·5	−21·6	Two-prism train.
Nov. 2	W.C.	2	b_1	+16·3	+ 0·2	− 4·8	Two-prism train.
2	M.	1½	b_1	+16·3	−24·9	−23·5	Two-prism train.
1879, Aug. 25	M.	6	b_1	+ 0·6	+ 7·3	+14·2	Definition fair.
1880, Sept. 29	M.	2	b_1	−10·2	−17·2	−19·0	Faint and tremulous.
				η Pegasi.			
1879, Aug. 25	M.	4	b_1	− 5·4	−23·7	−23·5	Definition bad.
1880, Sept. 27	M.	1	b_1	+ 2·9	−10·3	−11·4	Faint. Doubtful.
				β Pegasi.			
1880, Sept. 27	M.	2	b_4	+ 2·1	+21·7	+23·2	Definition fair.
27	M.	2	b_1	+ 2·1	+ 8·6	+10·6	
				α Pegasi.			
1880, Sept. 3	M.	2	F	− 2·9	−21·5	−20·9	Definition bad.
Nov. 25	W.C.	3	F	+17·0	−77·1	−51·4	
27	M.	13	F	+17·2	−39·9	−50·0	

The following table gives the mean results of all the observations of star motions at Greenwich compared with the results obtained by Dr. Huggins. The probable errors of the Greenwich results, inferred from the separate discordances, are appended with the sign ±, in cases where there were sufficient observations to give a rough estimate of the probable error. The character of the line measured is given in the remarks, as on this depends very largely the accuracy of the observations.

Motions of Stars in the Line of Sight in Miles per Second.

+ denotes recession; − denotes approach. The weights are on a scale ½ to 5 for each observation.

Star's Name.	Concluded Motion.	Weight.	No. of Nights.	Dr. Huggins' Results.	Remarks.
α Andromedæ	−33 ± 4·2	28	9	−	F broad; nebulous.
β Cassiopeiæ	+?	7	1		F broad; diffused.
γ Pegasi	−46	18	3	−?	F diffused.
α Cassiopeiæ	+?	9	2		b_1
γ Cassiopeiæ	−27	12	2	+	F bright; nebulous.
β Andromedæ	+12	18	2		b_1 distinct.
β Arietis	−14	11	2		F broad; v. dark.
α Piscium	−36	7½	2		F narrow; sharp.
γ¹ Andromedæ	−?	10	2		b_1
α Arietis	−21	11	4		b_1 distinct.
α Ceti	−34?	4	1		b_1, b_4; fluted spectrum.
β Persei	−	3	1		F v. dark; sharp.
α Persei	+25.	7	2		F faint; diffused.
ζ Persei	−	1	1		b_1 v. faint.
Aldebaran	+20 ± 2·0	26½	7	+?	b_1 v. distinct.
Capella	+27 ± 4·5	53	9	±	b_1 distinct; F sharp.
Rigel	+18 ± 1·9	40½	9	+15	F v. distinct.
γ Orionis	− 1 ± 1·4	25	4		F v. distinct.
β Tauri	−18	17	3		F broad; diffused.
δ Orionis	+ 4 ± 6·1	11½	6		F faint; narrow.
ε Orionis	−22 ± 10·3	23	6		F faint; narrow.
ζ Orionis	+ 9	7	3		F faint; narrow.
κ Orionis	− 1	4	2		F distinct.
α Orionis	+21 ± 1·8	54	8	+22	b_1, b_4; fluted spectrum.
β Aurigæ	− 7	12	3		F v. broad; neb.
γ Geminorum	+ 2	9	2		F broad; sharp.
Sirius	+20 ± 2·4	81	10	+18 to 22	F v. broad; diffused.
β Canis Minoris	−?	3	1		F broad; diffused.
Castor	+25 ± 4·2	34	8	+23 to 28	F v. broad; diffused.
Procyon	+24 ± 3·9	65	10	+	F broad; nebulous.
Pollux	−26 ± 4·0	74	13	−49	b_1 dist. F faint; sharp.
α Hydræ	+39	7½	2		b_1
ε Leonis	−14	8	3		b_1 distinct.
Regulus	+26 ± 2·4	48	8	+12 to 17	F broad.

Star's Name.	Concluded Motion.	Weight.	No. of Nights.	Dr. Huggins' Results.	Remarks.
γ^1 Leonis	$-38 \pm 4{\cdot}6$	25	5	−	b_1
β Ursæ Majoris	$+28 \pm 4{\cdot}7$	34	8	+17 to 21	F v. dark; brd.; neb.
α Ursæ Majoris	−27	6	2	−46 to 60	b_1; F faint.
δ Leonis	−28	14	2	+	F broad; dark.
θ Leonis	+?	2	1		F v. dark; brd.; diff.
χ Ursæ Majoris	+?	1	1		F v. dark; brd.; neb.
β Leonis	+34	18	4	+	F broad.
γ Ursæ Majoris	$+17 \pm 2{\cdot}7$	30	6	+17 to 21	F v. dark; brd.; neb.
δ Ursæ Majoris	+10	6	2	+17 to 21	F v. dark; brd.; neb.
γ Virginis	+37	10	4		F faint; diffused.
ϵ Ursæ Majoris	$+16 \pm 3{\cdot}7$	27	5	+17 to 21	F broad; diffused.
δ Virginis	+?	1	1		b_4; fluted spectrum.
α Canum Venat.	−?	8	1		F v. dark; broad;
ϵ Virginis	−28	4	2		b_1 [sharp.
Spica	$+4 \pm 4{\cdot}0$	36	7	+	F narrow; sharp.
ζ Ursæ Majoris	+18	16	3	+17 to 21	F v. dark; brd; neb.
η Ursæ Majoris	−8	13	3	+	F broad; diffused.
η Boötis	−?	4	1		b_1 distinct.
Arcturus	$-33 \pm 2{\cdot}8$	110	14	−55	b_1 v. distinct; D_3 dark; [F sharp.
γ Boötis	−?	2	1		F broad.
ϵ^2 Boötis	$-7 \pm 3{\cdot}6$	36	7	−	b_1, b_4 distinct.
β Ursæ Minoris	+26	8	2		b_1 faint.
β Libræ	+11	18	3		F faint; sharp.
α Coronæ	$+40 \pm 4{\cdot}6$	35	7	+	F v. dark; brd; diff.
α Serpentis	−?	3	1		b_4; fluted spectrum.
ϵ Serpentis	+35	10	2		F broad.
γ Herculis	−?	2	2		F diffused.
η Draconis	+3	8½	3		b_1 v. faint.
β Herculis	−?	4½	2		b_1 v. faint.
ζ Herculis	+25	7	2		b_1
ζ Draconis	−22	5	2		F diffused.
α Herculis	−31	5½	2		b_4 fluted spectrum.
β Draconis	+20	9½	4		b_1, b_4 v. faint.
α Ophiuchi	$+17? \pm 7{\cdot}1$	38	9		F v. diffused.
ξ Draconis	+?	1	1		b_1 v. faint.
γ Draconis	$-17 \pm 2{\cdot}0$	18½	5		b_1, b_4 v. distinct.
α Lyræ	$-34 \pm 2{\cdot}4$	126	14	−44 to 54	F v. dark; brd.; diff.

Star's Name.	Concluded Motion.	Weight.	No. of Nights.	Dr. Huggins' Results.	Remarks.
γ Lyræ	+ 9	14	2		F broad; diffused.
ζ Aquilæ	−16?±16·5	27½	5		F v. broad; diffused.
δ Aquilæ	−?	1	1		F broad.
β Cygni	−18	10	3		b_1 distinct.
γ Aquilæ	−14	7	3		b_1 distinct.
δ Cygni	−17 ± 3·6	25	5		F diffused.
α Aquilæ	−13?±11·8	21	9		F v. broad; diffused.
γ Cygni	−15 ± 3·4	37	9	−	b_1, b_4 dist.; F sharp.
α Delphini	+22	3	2		F v. broad.
α Cygni	−43 ± 4·4	51	9	−39	b_1 faint; F sharp.
ϵ Cygni	+18	12½	4		b_1, b_4
ζ Cygni	−?	1	1		b_1 indistinct.
α Cephei	−?	2	1		F v. dark; sharp.
β^2 Cephei	+?	3	1		b_1 indistinct.
ϵ Pegasi	−15 ± 2·5	32	10		b_1, b_2, b_4 distinct.
η Pegasi	−21	5	2		b_1
Fomalhaut	?	9	2		F distinct.
β Pegasi	+19	14	3		b_1, b_4; fluted spectrum.
α Pegasi	−34 ± 5·9	34	7	−	F broad; distinct.

Royal Observatory, Greenwich,
1881, *January* 13.

Observations of Occultations of Stars by the Moon, of Phenomena of Jupiter's Satellites, and of the Eclipse of the Sun, 1880, December 31, made at the Royal Observatory, Greenwich, in the Year 1880.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs.	Phenomenon.	Telescope.	Power.	Moon's Limb.	Mean Solar Time of Obs. h. m. s.	Observer.
1880, Jan. 20	(*a*) Disapp. of ϵ Arietis	E. Eq.	140	Dark	7 4 27·7	A. D.
March 13	Disapp. of 101 Piscium	,,	,,	,,	7 34 36·6	C.
21	Disapp. of d^2 Cancri	,,	,,	,,	7 16 25·6	,,
24	(*b*) Disapp. of p^2 Leonis	,,	,,	,,	13 35 54·4	,,
April 16	Disapp. of 56 Geminorum	Altaz.	100	,,	10 43 0·9	T.
May 18	(*c*) Disapp. of p^2 Leonis	E. Eq.	140	,,	7 56 58·1	C.
Sept. 13	Disapp. of 50 Sagittarii	Altaz.	100	,,	7 14 51·5	,,
Nov. 17	(*d*) Disapp. of ν' Tauri	,,	,,	Bright	6 54 39·9	,,
	Disapp. of ν' Tauri	E. Eq.	140	,,	6 54 39·4	A. P.
19	Disapp. of η Geminorum	,,	,,	,,	8 52 4·3	C.
	(*e*) Reapp. of η Geminorum	,,	,,	Dark	9 58 12·5	,,
	Disapp. of μ Geminorum	,,	,,	Bright	13 39 40·3	,,
	Reapp. of μ Geminorum	,,	,,	Dark	14 43 10·3	,,
20	Disapp. of ζ Geminorum	Altaz.	100	Bright	8 16 40·8	J. P.
	Reapp. of ζ Geminorum	,,	,,	Dark	9 14 31·4	,,

Notes.

(*a*) The star seemed to disappear gradually.
(*b*) Images very bad; observation doubtful, as the observer lost sight of the star several times before it finally disappeared.
(*c*) Observation satisfactory.
(*d*) The observer lost sight of the star once or twice when close to the limb.
(*e*) The reappearance was not sudden; the star was first seen as a very small spot, and in about $0^s{\cdot}2$ or $0^s{\cdot}3$ it flashed out to its full brightness.

Phenomena of Jupiter's Satellites.

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Mean Solar Time of N.A.	Observer.
					h m s	h m s	
1880, Aug. 2	I.	Occ. reapp. First seen	E. Eq.	140	12 44 9	12 45	C.
		Last contact	,,	,,	12 45 22		
2	(*a*) III.	Tr. egr. Bisection	,,	,,	12 59 10	13 6	,,
		Last contact	,,	,,	13 2 24		
9	III.	Tr. egr. First contact	,,	,,	14 32 57	14 34	J.
		Bisection	,,	,,	14 39 26		
		Last contact	,,	,,	14 46 15		
9	I.	Occ. reapp. First contact	,,	,,	14 33 57	14 34	,,
		Bisection	,,	,,	14 34 52		
		Last contact	,,	,,	14 35 56		
31	II.	Occ. reapp. Bisection	,,	,,	12 55 51	12 58	T.
		Last contact	,,	,,	12 58 20		
Sept. 1	I.	Ecl. disapp. First obscuration	,,	310	11 18 41	11 20 41	A. D.
		Last seen	,,	,,	11 20 43		
2	I.	Tr. ingr. First contact	,,	,,	9 27 49	9 29	C.
		Bisection	,,	,,	9 30 3		
		Last contact	,,	,,	9 32 3		

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Sept. 2	I.	Tr. egr. First contact	E. Eq.	310	11 35 43	11 42	C.
		Bisection	,,	,,	11 38 27		
		Last contact	,,	,,	11 40 27		
3	III.	Ecl. disapp. Last seen	,,	,,	11 17 24	11 17 25	T.
	(*b*) III.	Ecl. reapp. First seen	,,	,,	13 44 29		
		Full brightness	,,	,,	13 45 56	13 47 36	,,
7	II.	Ecl. disapp. Last seen	,,	,,	11 11 54	11 11 1	H. P.
16	(*c*) II.	Tr. ingr. First contact	,,	,,	9 10 57	9 12	A. D.
		Last contact	,,	,,	9 16 18		
Oct. 2	(*d*) I.	Tr. ingr. First contact	,,	140	10 48 34	10 51	,,
		Last contact	,,	,,	10 54 18		
2	(*e*) II.	Occ. reapp. First contact	,,	,,	11 1 31	11 5	,,
		Last contact	,,	,,	11 8 20		
2	(*f*) I.	Tr. egr. First contact	,,	,,	13 0 12	13 4	,,
		Last contact	,,	,,	13 5 6		
11	(*g*) I.	Tr. ingr. First contact	,,	,,	5 59 30	7 0	,,
		Last contact	,,	,,	7 3 21		
11	(*h*) II.	Tr. egr. First contact	,,	,,	7 39 59	7 46	,,
		Last contact	,,	,,	7 44 33		
11	(*i*) I.	Tr. egr. First contact	,,	,,	9 8 39	9 13	,,
		Last contact	,,	,,	9 13 8		

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Oct. 18	(*k*) II.	Tr. ingr. First contact	E. Eq.	140	7 24 9		
		Bisection	,,	,,	7 25 39	7 25	C.
		Last contact	,,	,,	7 27 38		
18	I.	Tr. ingr. First contact	,,	,,	8 43 56		
		Bisection	,,	,,	8 45 26	8 44	,,
		Last contact	,,	,,	8 47 25		
18	(*l*) II.	Tr. egr. First contact	,,	,,	9 54 59		
		Bisection	,,	,,	9 57 44	10 2	,,
		Last contact	,,	,,	10 0 13		
18	I.	Tr. egr. Last contact	,,	,,	10 56 4	10 57	,,
25	(*m*) I.	Tr. egr. First contact	S. E. Eq.	130	12 36 3		
		Bisection	,,	,,	12 40 12	12 41	M.
		Last contact	,,	,,	12 44 11		
Nov. 2	I.	Ecl. reapp. First seen	E. Eq.	140	12 18 3	12 17 50	J.
3	II.	Occ. disapp. First contact	Naylor	215	6 28 45		
		Bisection	,,	,,	6 31 15	6 30	W. C.
		Last contact	,,	,,	6 33 15		
3	II.	Occ. disapp. First contact	E. Eq.	310	6 30 25		
		Bisection	,,	,,	6 31 55	6 30	C.
		Last contact	,,	,,	6 33 24		

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Sola Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Nov. 3	(*) I.	Tr. ingr. First contact	Naylor	215	6 39 44		
		Bisection	,,	,,	6 41 3	6 39	W. C.
		Last contact	,,	,,	6 43 13		
3	I.	Tr. ingr. First contact	E. Eq.	310	6 38 38		
		Bisection	,,	,,	6 40 53	6 39	C.
		Last contact	,,	,,	6 42 38		
3	III.	Tr. ingr. First contact	Naylor	215	6 54 30		
		Bisection	,,	,,	6 59 39	6 54	W. C.
		Last contact	,,	,,	7 3 29		
3	(*o*) III.	Tr. ingr. First contact	E. Eq.	310	6 52 51		
		Bisection	,,	,,	6 58 50	6 54	C.
		Last contact	,,	,,	7 6 4		
3	I.	Tr. egr. First contact	,,	,,	8 47 32	8 52	,,
		Last contact	,,	,,	8 51 47		
3	III.	Tr. egr. Bisection	Naylor	215	9 14 41	9 21	W. C.
		Last contact	,,	,,	9 21 40		
3	III.	Tr. egr. Bisection	E. Eq.	310	9 15 58	9 21	C.
		Last contact	,,	,,	9 20 42		
3	(*p*) II.	Ecl. reapp. First seen	,,	,,	10 27 49	10 28 43	,,

Day of Observation.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Mean Solar Time of N. A.	Observer.
					h m s	h m s	
Nov. 4	(*q*) I.	Ecl. reapp. First seen	S. E. Eq.	130	6 46 18	6 46 42	M.
	I.	Ecl. reapp. First seen	E. Eq.	310	6 45 57	6 46 42	T.
		Full brightness	,,	,,	6 47 27		
10	I.	Tr. ingr. First contact	Naylor	215	8 23 19		
		Bisection	,,	,,	8 25 29	8 25	M.
		Last contact	,,	,,	8 27 48		
10	II.	Occ. disapp. First contact	,,	,,	8 46 34		
		Bisection	,,	,,	8 48 44	8 47	,,
		Last contact	,,	,,	8 50 9		
19	(*r*) I.	Tr. egr. Last contact	E. Eq.	310	6 53 26	6 53	C.
19	II.	Tr. egr. First contact	,,	,,	8 27 11		
		Bisection	,,	,,	8 29 11	8 34	,,
		Last contact	,,	,,	8 30 40		
21	(*s*) II	Ecl. reapp. First seen	Naylor	110	4 56 34	4 58 4	W. C.
		Full brightness	,,	,,	4 59 3		
21	III.	Occ. reapp. Last contact	,,	215	5 48 22	5 57	,,
21	III.	Ecl. disapp. First obscuration	,,	,,	7 36 0	7 43 23	,,
	(*t*) III.	Last seen	,,	,,	7 44 4		

Day of Observation.	Satellite.	Phenomenon.	Telescope	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N. A. h m s	Observer.
Nov. 21	(*u*) III.	Ecl. reapp. First seen	"	"	9 53 23	9 58 24	W. C.
		Full brightness	"	"	10 1 2		
21	III.	Ecl. reapp. First seen	E. Eq.	310	9 53 12	9 58 24	C.
		Full brightness	"	"	9 59 5		
27	I.	Ecl. reapp. First seen	"	"	7 2 39	7 2 56	"
27	I.	Ecl. reapp. First seen	Naylor	215	7 2 26	7 2 56	M.
28	(*v*) III.	Occ. disapp. First contact	"	"	6 49 2	6 58	W. C.
		Bisection	"	"	6 53 12		
		Last contact	"	"	7 1 20		
28	II.	Ecl. reapp. First seen	"	"	7 32 33	7 34 3	"
		Full brightness	"	"	7 36 3		
28	(*w*) III.	Occ. reapp. First seen	"	"	9 25 11	9 35	"
		Last contact	"	"	9 30 40		
Dec. 3	(*x*) I.	Tr. egr. First contact	E. Eq.	149	10 27 55	10 31	A. D.
		Last contact	"	"	10 33 25		
3	(*y*) II.	Tr. ingr. First contact	"	"	10 39 54	10 47	"
		Last contact	"	"	10 48 37		
Dec. 20	(*z*) I.	Ecl. reapp. First seen	Altaz.	100	7 20 31	7 19 32	C.
30	II.	Ecl. reapp. First seen	E. Eq.	140	7 17 46	7 17 51	J.

Notes.

(*a*) Images very blurred and unsteady.

(*b*) The planet and satellites were ill-defined.

(*c*) Limb ill-defined.

(*d*) The images were diffused and tremulous and the observation difficult. The satellite could be traced for a quarter of an hour on the disk of Jupiter, and was distinctly brighter than the surface of the planet near the limb.

(*e*) Images diffused and tremulous.

(*f*) Tremulous; light cloud.

(*g* and *h*) Ill-defined and tremulous.

(*i*) Steady, but diffused.

(*k*) The satellite when on the disk of Jupiter appeared brighter than the surrounding surface.

(*l*) The shadows of the satellites were intensely black.

(*m*) Limb of planet tremulous; light clouds passing.

(*n*) The satellite was much brighter than Jupiter's disk near the limb. About eleven minutes after the last contact it was invisible on the planet.

(*o*) The satellite was seen bright on Jupiter's disk about seven minutes after the last contact, and barely visible eleven minutes later.

(*p*) Definition very bad.

(*q*) There was a considerable amount of fog, which probably prevented the first speck of light being recognised at once. The images were very steady.

(*r*) Not good; very cloudy.

(*s*) and (*t*) Observation good.

(*u*) A few seconds late.

(*v*) Sky hazy; definition fair. The satellite was seen very distinctly through the edge of the planet's disk.

(*w*) Definition bad.

(*x*) The satellite faint; the sky thick.

(*y*) The satellite faint; observation difficult.

(*z*) Planet faint on account of fog; the observation not satisfactory.

Eclipse of the Sun, 1880, December 31.

Beginning and Ending of the Eclipse.

		Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation.	Observer.
					h m s	
1880, Dec. 31	(*a*)	First Contact	S.E. Eq.	295	1 40 33·6	W. C.
,,	(*b*)	,,	E. Eq.	70	1 40 32·9	D.
,,		,,	Naylor	215	1 40 35·8	A. D.
,,	(*c*)	,,	Altazimuth	66	1 40 37·4	T.
,,	(*d*)	Last Contact	S.E. Eq.	295	3 28 23·0	W. C.
,,	(*e*)	,,	E. Eq.	140	3 28 19·3	C.

Notes.

(*a*) Probably a few seconds late.

(*b*) At the time of contact the Sun's limb was steady and free from cloud. Clouds were, however, passing over the Sun till about 20 seconds before first contact, occasionally obscuring the limb.

(*c*) Clouds only just left the sun a few seconds before first contact.

(*d*) Observation pretty good.

(*e*) Very tremulous.

During the Eclipse the following observations were made with the S. E. Equatoreal:—

From 1 h 40 m to 1 h 55 m G.M.T.,	10 differences of N.P.D. of N. and S. cusps.
1 55 to 2 15	10 differences of R.A. of *p* and *f* cusps.
2 15 to 2 40	13 differences of N.P.D. of S limbs of Sun and Moon.
2 40 to 3 0	11 differences of N.P.D. of *p* and *f* cusps.
3 0 to 3 25	12 differences of R.A. of *p* and *f* cusps.

The clear aperture of the object-glass of the S.E. Equatoreal is $12\frac{3}{4}$ inches, of the East Equatoreal $6\frac{3}{4}$ inches, of the Altazimuth 4 inches, and of the Naylor Equatoreal 6 inches.

The initials W. C., D., C., A. D., M., T., H. P., J. P., J., and A. P., are those of Mr. Christie, Mr. Dunkin, Mr. Criswick, Mr. Downing, Mr. Maunder, Mr. Thackeray, Mr. H. Pead, Mr. Power, Mr. James, and Mr. A. Pead.

Royal Observatory, Greenwich,
1881, *January* 5.

Occultations of Stars by the Moon seen at the Stonyhurst Observatory. By the Rev. S. J. Perry.

		Phenomenon.	Moon's Limb.	G. M. T. h m s	Observer.	Remarks.
1879, Dec.	22	Disapp. of 101 Pisc.	dark	5 3 35·0	S. J. P.	Instantaneous. Double Lunar burr.
	25	„ „ 36 Tauri	„	6 0 44·9	W. C.	Good. Instantaneous.
	26	„ „ κ Tauri		6 29 35·1	S. J. P.	Obsn. very difficult. Star very faint.
		Reapp. „ „		7 24 39·4	„	Rather late.
1880, Jan.	20	Disapp. „ ε Arietis	dark	7 4 43·9	W. C.	Very good. Instantaneous.
		Reapp. „ „	bright	8 23 50·6	„	Good. Instantaneous.
	28	Disapp. „ B.A.C. 3407	„	10 7 21·9	S. J. P.	Difficult to see when close to Moon.
		Reapp. „ „	dark	11 10 29·6	„	Star rather faint, perhaps somewhat late. Haze.
	28	Disapp. „ π Leonis	bright	11 29 8·8	„	Good. Limb unsteady.
		Reapp. „ „	dark	12 31 47·4	„	Excellent.
March	13	Disapp. „ 101 Pisc.	„	7 29 54·4	W. C.	Excellent. Dark limb very distinct. Star appeared to grow slightly fainter about 0ˢ·5 before disappearance.
	17	Disapp. „ 103 Tauri	„	12 27 31·7	„	Very good, slight haze round Moon.
	18	„ „ 132 Tauri	„	6 12 23·9	„	Very good. Instantaneous.
		Reapp. „ „	bright	7 16 3·9	„	Excellent.
	21	Disapp. „ d^2 Cancri	dark	7 15 33·5	S. J. P.	Instantaneous.
		Reapp. „ „	bright	8 14 46·9	„	Good. Star faint.
	25	Disapp. „ B.A.C. 4006	dark	11 4 51·9	W. C.	Very good. Moon nearly full.
		Reapp. „ „		12 3 26·6	„	Nearly 1ˢ late.

		Phenomenon.	Moon's Limb.	G. M. T. h m s	Observer.	Remarks.
April	14	Disapp. „ 121 Tauri	dark	8 32 27·8	W. C.	Sky hazy, thin clouds passing. Good.
	16	„ „ 56 Geminorum	„	10 37 52·5	„	Instantaneous. Defn. very good. Limb visible.
		Reapp. „ „	bright	11 21 46·8	„	Very good.
May	16	Disapp. „ λ Leonis	dark	10 42 3·6	S. J. P.	Very good. Instantaneous.
Sept.	13	„ „ 50 Sagitt.	„	7 10 49·7	W. C.	Good.
		Reapp. „ „	bright	8 5 5·5	„	Very good.
Oct.	10	Disapp. „ B.A.C. 6561	„	8 23 15·6	S. J. P.	Approx. Fair. } Passed terminator 3m 37s·5 bef. disapp.
		Reapp. „ „	„	8 32 54·1	„	Defn. poor, unsteady. }
	19	Disapp. „ ζ Arietis	„	17 1 50·3	W. C.	Screen used. Well seen right up to limb. Instantaneous.
	20	„ „ 32 Tauri	„	10 27 42·2	„	Screen used. Very good.
		Reapp. „ „	dark	11 30 48·7	„	Screen used. Very good.
Nov.	19	Disapp. „ η Geminorum	bright	8 54 34·9	„	Excellent. Screen used.
		Reapp. „ „	dark	9 59 59·4	„	Excellent. Screen used.
		Disapp. „ μ Geminorum	bright	13 41 6·5	„	Excellent. Screen used.
	20	„ „ ζ² Geminorum	„	8 19 49·0	„	Sky slightly hazy, but observation very good.
		Reapp. „ „	dark	9 18 31·1	„	Very good, screen used. Sky improving.
		„ „ ζ¹ Geminorum	„	9 18 7·4	„	Very good.
	21	Disapp. „ 3 Cancri	bright	12 34 22·5	„	Difficult, owing to passing clouds.
		Reapp. „ „	dark	13 40 33·7	„	Excellent.

Observations of Jupiter's Satellites made at the Stonyhurst Observatory. By the Rev. S. J. Perry.

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
1879, Dec. 12	II.	Oc. D. First contact	4 56 5·6		W. McK.	Defin. good at times. Thin clouds passing.
		Bisection	58 10·2			
		Last seen	5 0 32·5			
23	IV.	Sh. E. Int. contact	6 38 14·4		S. J. P.	Limb boiling. Defin. fair at centre. Sh. very black at edge of γ. Obs. indifferent.
		Bisection	49 57·0			
	I.	Oc. D. Ext. contact	7 47 32·4		,,	Defin. very bad.
		Bisection	49 57·0			
24	I.	Tr. I. Ext. contact	4 57 28·5		W. McK.	At bisection thin clouds; at internal contact sky very clear.
		Bisection	5 0 41·1			
		Int. contact	4 1·9			
	I.	Sh. I. Bisection	6 16 19·7		W. C.	Hazy and slightly tremulous. Sh. very black.
		Int. contact	17 37·2			
	I.	Tr. E. Bisection	7 20 8·3		,,	Satellite exceedingly faint on planet. Unsteady.
		Ext. contact	22 21·3			
1880, Jan. 22	II.	Tr. E. Bisection	6 15 38·7		,,	Satellite very faint off disk. Unsteady.
		Ext. contact	17 39·8			
Sept. 8	I.	Ec. D. Last seen	13 16 10·3	+0 52·3	,,	Very good.
28	III.	Sh. I. Bisection	13 33 29·2			Good. Sh. very black, but very small.
		Int. contact	35 50·7			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Sept. 28	„	Tr. I. Ext. contact	14 34 27·9		W. C.	Sky very misty, defin. rather poor. Sh. still very black.
		Bisection	38 54·4			
		Int. contact	44 44·4			
28	„	Sh. E. Int. contact	15 59 52·5		„	Sky hazy, but defin. fairly good.
		Bisection	16 3 9·0			
		Last seen	7 11·5			
Oct. 2	II.	Ec. D. Last seen	8 14 45·7	+0 37·7		Rather difficult owing to brilliancy of planet.
2	I.	Sh. I. Bisection	10 45 30·6		„	Rather faint. Defin. good.
		Int. contact	47 35·4			
2	„	Tr. I. Ext. contact	10 51 7·0		„	Hazy. Defin. rather poor.
		Bisection	52 31·7			
		Int. contact	54 52·4			
2	„	Tr. E. Bisection	13 1 40·5		„	Hazy. Defin. poor.
		Ext. contact	4 11·9			
3	„	Ec. D. Last seen	7 57 7·5	−0 18·5	S. J. P.	Fair, limb boiling, satellite close to limb.
3	„	Oc. R. First seen	10 12 50·1		„	Good, limb a little unsteady. Defin. very good.
		Bisection	14 50·4			
		Ext. contact	16 34·6			
8	„	Oc. D. Ext. contact	15 16 30·1		W. C.	Defin. good, rather unsteady.
		Bisection	18 31·6			
		Last seen	20 35·6			

		Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Oct.	9	III.	Oc. D. Ext. contact	7 8 37·7		W. C.	Poor, very hazy, thin clouds.
			Bisection	11 46·9			
			Last seen	14 52·1			
	9	,,	Ec. R. First seen	9 47 0·2	−3 18·8	,,	Good, full brilliancy 4m later. Rather difficult owing to satellite being so near ♃.
	9	II.	Oc. D. Ext. contact	10 39 28·5		,,	Slight haze round ♃. Slightly tremulous
			Bisection	41 27·0			but defin. good.
			Last seen	43 35·4			
	9	I.	Tr. I. Ext. contact	12 32 55·6		,,	Defin. poor, tremulous.
			Bisection	35 12·4			
			Int. contact	36 52·1			
	9	,,	Sh. I. Bisection	12 39 27·7		,,	Tremulous. Sh. faint.
			Int. contact	42 3·2			
	9	II.	Ec. R. First seen	13 24 42·7	−0 21·3	,,	Very good.
	9	I.	Tr. E. Int. contact	14 42 25·9			Defin. good. Tremulous. Sh. too faint to
			Bisection	45 12·7			observe in so unsteady a sky.
			Ext. contact	47 57·9			
	10	,,	Oc. D. Ext. contact	9 41 1·2		S. J. P.	Defin. poor. Satellite projected on limb
			Bisection	45 24·7			during occultation. Limb very tremulous
			Last seen	47 39·7			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Oct. 10	I.	Ec. R. First seen	12 4 7·1	+1 25·1	W. C.	Good. Full light only approx.
		Full light	6 19·3			
11	„	Tr. I. Ext. contact	6 55 2·0		S. J. P.	Satellite very bright on disk. Obs. only fair, misty.
		Bisection	57 0·6			
		Int. contact	7 0 43·8			
11	„	Tr. E. Int. contact	9 6 27·1		„	Defin. good, but passing clouds.
		Bisection	10 12·3			
		Ext. contact	13 6·0			Last contact seen through thin clouds. Sh. very small, fairly distinct at times, but clouds too frequent for good obs. of egress of Sh.
19	„	Oc. D. Ext. contact	5 53 23·7		W. C.	Only fair, defin. poor, tremulous.
		Bisection	54 56·7			
		Last seen	56 34·2			
19	„	Ec. R. First seen	8 26 44·3	−0 8·7	„	Fair. Screen used.
		Full light	29 51·9			
23	III.	Oc. D. Ext. contact	13 40 23·8		„	Defin. good, slightly unsteady.
		Bisection	44 40·6			
		Last seen	49 9·0			
23	II.	Oc. D. Ext. contact	15 4 42·7		„	Defin. very poor, tremulous.
		Bisection	7 11·9			
		Last seen	9 2·6			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Nov. 1.	II.	Tr. I. Ext. contact	11 56 2·6		W. C.	Defin. not very good.
		Bisection	58 11·8			
		Int. contact	12 0 31·5			
1	I.	Tr. I. Ext. contact	12 9 42·0		„	Defin. poor, unsteady.
		Bisection	10 30·6			
		Int. contact	13 4·8			
1	„	Sh. I. Bisection	12 54 26·6			Fair, defin. improved.
		Full on	56 26·2			
1	II.	Sh. I. Bisection	13 16 21·9		W. C.	Defin. bad. Sh. small and faint.
		Full on	19 8·1			
1	I.	Tr. E. Int. contact	14 21 41·0		„	Defin. fair, tremulous.
		Bisection	23 54·1			
		Ext. contact	26 35·3			
1	II.	Tr. E. Int. contact	14 34 38·8		„	Very unsteady.
		Bisection	36 26·2			
		Ext. contact	38 12·0			
3	„	Oc. D. Ext. contact	6 26 47·6		„	Good.
		Bisection	30 11·1			
		Last seen	33 24·6			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks.
Nov. 3	I.	Tr. I. Ext. contact	6 37 58·6		W. C.	Good.
		Bisection	40 40·3			
		Int. contact	43 6·1			
3	III.	Tr. I. Ext. contact	6 47 51·6		„	Good.
		Bisection	52 34·6			
		Int. contact	57 50·1			
3	I.	Sh. I. Bisection	7 21 38·2		„	Defin. fair. Sh. exceedingly faint, very difficult observation.
		Full on	23 24·2			
3	„	Tr. E. Int. contact	8 48 30·9		„	Defin. good, night improving.
		Bisection	50 38·4			
		Ext. contact	52 50·4			
3	III.	Tr. E. Int. contact	9 11 52·9		„	Defin. good, sky a little hazy, field white.
		Bisection	18 25·4			
		Ext. contact	23 55·0			
3	I.	Sh. E. Int. contact	9 29 39·5		„	Shadow faint, but obs. fair.
		Bisection	30 59·5			
		Last seen	32 52·5			
3	III.	Sh. I. Bisection	9 41 29·5		„	Defin. good. Sh. very small but black.
		Full on	44 41·5			
3	II.	Ec. R. First seen	10 26 40·0	−2 3·0	„	Very good. Sh. of III. very dark on disk.
		Full light	31 4·6			

	Satellite.	Phenomenon.	G. M. T. h m s	In excess of N. A. m s	Observer.	Remarks
Nov. 3	III.	Sh. E. Int. contact	12 3 26·8		W. C.	Very poor, defin. bad, unsteady.
		Bisection	6 55·8			
		Last seen	10 24·8			
19	II.	Tr. I. Ext. contact	5 47 36·2		„	Very poor. Thin clouds passing.
		Bisection	49 55·7			
		Int. contact	52 23·2			
19	I.	Tr. E. Int. contact	6 48 48·6		,	Good, night much better.
		Bisection	51 2·3			
		Ext. contact	53 25·1			
19	II.	Tr. E. Int. contact	8 26 6·2		„	Rather tremulous, but obs. very good.
		Bisection	29 18·9			
		Ext. contact	31 36·4			
20	I.	Ec. R. First seen	5 6 47·1	−0 19·9	„	
21	II.	Ec. R. First seen	4 56 25·5	−1 38·5	„	Sky hazy, full light 3m later.
21	III.	Oc. R. Bisection	5 51 4·8			Tremulous, but obs. good.
		Ext. contact	54 58·8			
27	I.	Ec. R. First seen	7 2 36·3	−0 19·7	„	Sky rather hazy.
		Full light	4 56·6			

Observations of the Solar Eclipse, 1880, *Dec.* 31, *made at the University Observatory, Strasburg.* By Dr. A. Winnecke.

The chances for getting some observations during the Solar eclipse of Dec. 31 were very poor; nevertheless we got up the Heliometer on the gallery of the building for the great Refractor, whence it was possible to follow the Sun to the horizon, the eclipse ending only a quarter of an hour before his setting.

Heavy clouds obscured the Sun during the first half of the eclipse, but then an opening coming up, we got 8 measures of the distances of the cusps with the Heliometer. After due reduction the chords measured by Dr. Schur and Dr. Hartwig were the following:

Mean Solar Time Strasburg.	Distance of Cusps.	Errors.
h m s	"	"
3 45 25·1	955·74	+1·03
46 34·1	935·59	−1·01
48 20·1	858·28	−1·37
49 58·1	814·43	−0·36
51 7·1	768·81	+2·57
52 3·1	704·80	−0·83
53 41·1	625·41	+1·23
54 39·1	581·59	−1·99

After the last measure clouds again covered the Sun. Using the Ephemeris of the Moon given by the "Connaissance des Temps," and correcting by $-1''\cdot90$ the semidiameter of the Sun and by $-2''\cdot30$ the semidiameter of the Moon,* and putting—

dr = correction to the adopted semidiameter of the Sun,

dR = " " " " Moon,

dx = " excess of Moon's R.A. over Sun's R.A. × cos δ,

dy = " " " Decl. over Sun's Decl.,

i = " " point of coincidence of the images formed by the two halves of the object glass,

c = error depending on the direction of moving the screws;

Dr. Schur puts the following equations of condition—

$0 = -$ 5·34	$+2\cdot094\,dr$	$+2\cdot076\,dR$	$-0\cdot429\,dx$	$-1\cdot178\,dy$	$+c+i$
$0 = -20\cdot71$	$+2\cdot174$	$+2\cdot158$	$-0\cdot482$	$-1\cdot859$	$-c+i$
$0 = -3\cdot81$	$+2\cdot327$	$+2\cdot311$	$-0\cdot577$	$-2\cdot011$	$+c-i$
$0 = -23\cdot24$	$+2\cdot511$	$+2\cdot497$	$-0\cdot684$	$-2\cdot191$	$-c-i$
$0 = -27\cdot88$	$+2\cdot680$	$+2\cdot656$	$-0\cdot775$	$-2\cdot355$	$-c-i$
$0 = -9\cdot71$	$+2\cdot850$	$+2\cdot838$	$-0\cdot864$	$-2\cdot518$	$+c-i$
$0 = -17\cdot47$	$+3\cdot264$	$+3\cdot253$	$-1\cdot069$	$-2\cdot911$	$+c+i$
$0 = -34\cdot90$	$+3\cdot627$	$+3\cdot618$	$-1\cdot241$	$-3\cdot253$	$-c+i$

* *Monthly Notices*, vol. xxxvi. p. 86.

Neglecting the corrections to the adopted semidiameters (they cannot be exactly made out by these measures), Dr. Schur obtained, by solving the equations by the method of least squares—

Correction to R.A. of the Moon $-13''.42$, with the prob. err. $5''.25$
„ Decl. „ $-3''.56$ „ $1''.61$

The probable error of a single measure is $\pm 1''.38$, very small considering the great zenith distance in which the measures had to be made.

The Heliometer has $0^m.076$ aperture and a focal distance of $1^m.15$; power 135.

1881, *Strasburg, Jan.* 5.

Eclipse of the Sun, Dec. 31, 1880, *observed at the University Observatory, Oxford.* By Prof. C. Pritchard.

The Solar eclipse of Dec. 31 was observed here under exceptionally good atmospheric circumstances, though the small altitude of the Sun interfered with the definition, and, towards the end of the eclipse, the roof of the University Museum obscured some portion of the Sun's disk.

Times of Contact.

	G. M. T.	Telescope.	Observer.	Method.
First contact	1 38 0·3	De La Rue reflector	Mr. Jenkins	E. & E.
„	1 38 2·6	Grubb refractor	Mr. Plummer	Chronograph.
Last contact	3 38 2·0	De La Rue reflector	Mr. Jenkins	E. & E.
„	3 28 3·7	Grubb refractor	Mr. Plummer	Chronograph.

The aperture of the refractor is 12½ inches, power employed 108
„ reflector is 13 „ „ 180

The distance of the cusps and the position angle of the line joining them were measured with a filar micrometer near the beginning and end of the eclipse. Six measures were secured at the beginning, and three towards the end. At present there has been no opportunity of reducing these measures, and if the results contain any point of interest they will be submitted to the Society.

Oxford,
1881, *Jan.* 13.

Observations of the Solar Eclipse of 1880, *December* 31, *made at the Radcliffe Observatory, Oxford.*
By E. J. Stone, Esq.

Phenomenon.	Instrument.	Clock or Chronometer.	Local Sidereal Time.	Greenwich Mean Solar Time.	Observer.	Reference.
			h m s	h m s		
Beginning of Eclipse	10-foot Equatoreal	Sidereal Chronometer	20 14 22·4	1 37 59·6	Mr. Wickham	(*a*)
Contact of Moon's limb with Penumbra of Sunspot	10-foot Equatoreal	Sidereal Chronometer	20 20 18·9	1 43 55·1	Mr. Wickham	
	42-inch Telescope	Sidereal Chronometer	20 20 19·4	1 43 55·6	Mr. Robinson	
Contact of Moon's limb with Umbra of Sunspot	10-foot Equatoreal	Sidereal Chronometer	20 20 43·7	1 44 19·9	Mr. Wickham	
	42-inch Telescope	Sidereal Chronometer	20 20 50·4	1 44 26·6	Mr. Robinson	
Ending of Eclipse	10-foot Equatoreal	Sidereal Chronometer	22 4 45·7	3 28 4·8	Mr. Wickham	(*b*)
	42-inch Telescope	Sidereal Chronometer	22 4 46·2	3 28 5·3	Mr. Robinson	(*c*)
	Heliometer	Heliometer Clock	22 4 45·2	3 28 4·3	Mr. Bellamy	(*d*)

Remarks.

(*a*) Limb tremulous, but observation good.

(*b*) Sun very low; thin cloud; observation fair.

(*c*) Observation good.

(*d*) Observed through cloud.

Mr. Luff, who was using a small telescope (with power 16, aperture 1 inch) and pocket solar chronometer, noted the following times:—

	h m s	
Beginning of Eclipse	1 38 5·4	Greenwich Mean Solar Time.
Contact with Mean Edge of Sunspot	1 44 11·4	

The Partial Eclipse of the Sun, December 31, 1880.

By J. Rand Capron, Esq.

This, though owing to bad weather not observable in the north, was mostly well seen in the southern parts of England. At Guildown a clouded morning was followed by a clear midday, and, for the time of year, a fairly bright afternoon, so that the phenomenon was visible throughout. The first contact (which, however, from the boiling of the edges of both Sun and Moon would probably have been uncertain) was missed; but a large oval spot lying in front of the Moon's edge, contact with the external edge of the penumbra of this spot was timed at $1^h\,45^m\,35^s$ G.M.T. Subsequently eight photographs of the partially eclipsed Sun's disk were taken with Wratten and Wainwright's dry instantaneous plates, upon the 6-inch Cooke Equatoreal. As to the first seven the image was enlarged with a Barlow lens, no allowance being made for difference in chemical and visual foci. Exposure was made by the sliding of plates of metal with slits of varying width cut in them. In the case of the last plate, the image was the primary one not enlarged. Exposure was purposely prolonged—no diaphragm being used, but the ordinary camera slide was opened and shut by hand as quickly as possible.

This plate shows the Sun's image reversed, white upon a dark grey ground. All the plates show with more or less distinctness a set of small spots in front of the Moon's limb, which is fairly well defined and serrated. The Moon's disk in contact with the Sun presents a bright line of light round it which I attribute to photographic irradiation.

I have obtained a similar result by photographing a black velvet disk suspended against a bright sky. The Moon's body was not observed external to the Sun's disk, but much mist was present even before first contact. No marked darkening of daylight was noticed as the result of the partial obscuration of the Sun, which set uneclipsed in a fine red glow. The times of exposure of the plates 3 to 8 were as under. The plate 8 shows the eclipse at very nearly total phase.

Times of Exposure of Plates.

	h	m	s
No. 1 } Not timed.			
2 }			
3	2	7	30
4	2	14	0
No. 5	2	18	0
6	2	25	0
7	2	28	30
8	2	30	0

Guildown, Guildford,
1881, *January* 10.

On some Difficulties connected with the Determination of the Diameter of Mars. By E. J. Stone, Esq.

Our determinations of the diameters of the planets are complicated by the existence of systematic personal errors in the observers and, to some extent, by constant errors depending upon the instrument used. But the determinations of the diameter of *Mars* which have been made from time to time, have differed so much amongst themselves that there are grounds for suspecting the existence of some source of systematic error which has been overlooked.

Thus, in the *Monthly Notices* for November, 1880, Mr. Downing has inferred, from a discussion of all the measures of the vertical diameter of *Mars* which have been made with the Greenwich Transit Circle, that Le Verrier's value of the angular diameter of *Mars*, at the Sun's mean distance, $11''\cdot10$, requires to be diminished to $9''\cdot70$. The results deduced from two extensive series of observations, with many changes amongst the observers, and with different adopted values of the diameter, are closely accordant; and the result has been correctly deduced from the observations on the assumptions made. But Le Verrier's result equally represents the observations on the assumption which he made.

The observations at Greenwich are made by a large number of persons; and, although some errors, of an unsystematic character, do thus creep into the work, and the discussion of the results is greatly complicated, the facilities afforded for the examination of questions connected with personal errors are very great.

I have collected, from the Greenwich volumes, the measures of the vertical diameters of *Mars* made 1851 to 1878, and assuming that the true value of the angular diameter $=\text{Tab.}(1+y)$, the personal errors of the observers are given on two suppositions.

TABLE I.

Personal Equations.

With Le Verrier's Diameter $11''\cdot10$ under the heading V.

With Downing's Diameter $9''\cdot697$ under the heading D.

Period 1851-1865.

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
	$''$	$''$	$''$	
Breen, J.	$-0\cdot29$	$+1\cdot35$	$-10\cdot40$	1
Breen, H.	$+0\cdot23$	$+2\cdot28$	$-13\cdot00$	2
Bowden	$+0\cdot42$	$+2\cdot53$	$-13\cdot40$	1
Carpenter, J.	$-1\cdot34$	$+0\cdot74$	$-13\cdot18$	15
Criswick	$-0\cdot83$	$+1\cdot54$	$-15\cdot01$	31

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
	″	″	″	
Christy, W. J.	−1·46	+0·14	−10·01	2
Chappell	+1·23	+3·87	−16·71	11
Davis	−1·84	+1·35	−20·20	4
Dolman	−1·25	+2·14	−21·45	2
Dunkin	−0·66	+1·39	−13·01	40
Eaton	+0·85	+3·92	−19·37	6
Ellis, W.	+1·09	+3·39	−14·60	54
Henry	+0·28	+2·17	−11·88	15
Henderson	+0·30	+2·06	−11·10	11
Kerschner	−0·86	+1·89	−17·43	12
Lynn	+1·40	+3·60	−13·92	12
Main	+0·80	+2·88	−13·20	1
Newcomb	+5·14	+7·03	−12·00	1
Nash	−0·89	+1·50	−15·33	3
Roberts	−1·11	+1·48	−16·38	8
Rogerson	+1·19	+3·15	−12·36	5
Stone	+1·73	+4·08	−14·85	6
Taylor, H.	+0·02	+1·90	−11·85	4
Taylor, F.	+0·31	+2·39	−13·20	1
Talmage	+3·76	+4·55	− 5·00	1
Wakelin	−1·62	+0·81	−15·40	4
Weighted Means	+0·06	+2·32	−14·32	253 = Sum

These coefficients of y require to be increased in the proportion of $\frac{11{\cdot}10}{8{\cdot}87}$ or 1·25 to be strictly comparable with those of Table II. This correction has been allowed for in the work.

TABLE II.

Personal Equations.

With Le Verrier's Diameter 11″·10 under the heading V.

With Downing's Diameter 9″·697 under the heading D.

Period 1866–1878.

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
	″	″	″	
Baker	+2·01	+3·73	−13·65	4
Bromley	−1·26	+2·19	−27·60	4
Christie, W. H. M.	−1·74	−0·04	−13·52	5
Criswick	−0·37	+1·78	−15·77	40
Carpenter, J.	−0·79	+0·95	−13·89	20
Carpenter, H.	+0·74	+2·67	−15·25	8

Observer's Name.	V.	D.	Factor of y.	Number of Observations.
	″	″	″	
Dennison	−0·94	+1·25	−17·43	3
Dunkin	−0·70	+1·09	−14·17	6
Downing	−0·14	+2·13	−18·04	26
Ellis	+1·12	+3·01	−15·00	23
Graham	+2·62	+5·03	−19·14	7
Harding	+0·15	+2·62	−19·60	1
Jenkins	+0·71	+2·96	−17·94	7
Keating	+0·18	+2·84	−21·10	2
Kerschner	+1·83	+3·68	−14·65	4
Laird	+1·63	+3·56	−15·28	5
Lynn	+2·34	+4·19	−14·73	18
Pett	−1·14	+1·27	−19·10	3
Pead	−2·03	+1·23	−25·90	3
Plummer	+1·15	+2·85	−13·50	2
Potts	+1·11	+3·16	−16·28	6
Power	+0·12	+2·39	−18·18	6
Pulley	−0·49	+1·44	−15·26	7
Robinson	+0·04	+2·56	−20·20	7
Thackeray	+0·81	+3·35	−20·13	21
Wickham	+0·06	+2·73	−21·15	2
Wright	−0·02	+1·92	−15·40	1
Weighted Means	+0·33	+2·47	−16·74	241 = Sum

It will be seen that the measures of different observers may differ by nearly 3″ in cases where the number of observations would render any mere errors of observation small, and where the factors of the adopted diameter are too nearly alike to render any adjustment of these differences by a change of the adopted diameter possible. A comparison of the results of the two series will also show that these personal differences remain, for the principal observers, almost unchanged, and that the methods of observing contacts adopted by experienced observers sensibly and systematically differ, or that real personal equations to a very sensible amount do, as a matter of fact, exist in the measures of the diameter of *Mars*.

The results given in these tables appear to me of some practical importance: they afford a very necessary caution against the acceptance of any measured diameters as definitive when all the measures have been made by the same person, unless a sufficient range exists amongst the measured diameters to allow of an accurate separation of the constant and variable parts of the observed diameter.

If we adopt Le Verrier's value, the mean error of all the observed diameters in Table (1) are destroyed, and the mean error in Table (2) is very small. It is clear, therefore, that Le Verrier has assumed that, in the mean of a large number of measures made by many different observers, the personal errors will mutually destroy each other's effects. The legitimacy of this assumption would appear to be confirmed by the smallness of the mean residual errors in both series of the Greenwich observations, although very considerable changes in the observing staff have taken place. But, of course, Le Verrier's assumption would be untrue, and his result vitiated to a corresponding extent, if there be any systematic source of error which affects the measured diameters of *all the observers* in the same way. Such a source of constant error is presented by telescopic irradiation.

The formula adopted by Mr. Downing is one which has previously been frequently adopted in similar discussions, and distinctly contemplates the existence of *constant* errors in the measures. Mr. Downing assumes that

$$x+y\,(\text{Tab. Diameter}) = \text{Obs. Diameter} - \text{Tab. Diameter},$$

and attempts to determine both x and y from the equations of condition. But, unless the observations are all made by the same person, or corrections are applied for personal errors, an assumption is virtually made that the measures of large and small diameters are equally distributed amongst the different observers. If this condition is satisfied, and the variations between the factors of y are sufficiently large to allow of an accurate separation of x and y, then the diameter thus determined has certainly greater claims to our acceptance than that found by putting $x=0$, which is really the form adopted by Le Verrier. I have separately discussed the observations made by the principal observers, who have observed diameters over a sufficient range; the results confirm the value of y obtained by Mr. Downing, and prove, as might be expected from the large number of observations employed, that the distribution of the measures has been sufficiently uniform to avoid any serious effects on the determination of y. But an inspection of Tables (1) and (2) shows that it is impossible to accept Mr. Downing's value of the diameter of *Mars* without accepting as a fact that the error of nearly 500 measures of the diameter of *Mars* made with an 8-inch telescope by 47 observers is more than 2″.

It appears to me very difficult to accept such a conclusion. The irradiation constant for the planet *Mars*, with such a telescope, can hardly amount to one-fifth of the quantity under consideration; and, although the mean personal error of the observers is included in the quantity, it is difficult to conceive that the mean personal error of so many observers can amount to nearly as large a quantity as the whole range, of personal error, which is well established amongst the large number of observers whose measures are compared in the present

paper. It appears to me, therefore, that there must be some systematic error which vitiates the result by rendering untrue the assumption that all deviations of the measured diameters from the formula

$$x + y\,(\text{Tab. Diameter}) = \text{Obs. Diam.} - \text{Tab. Diam.}$$

arise from mere chance errors of observation. If we represent the errors geometrically by points, whose abscissæ are the tabular diameters and ordinates the excess of observed diameters over the tabular diameters, we ought, on the suppositions which underlie our investigations, to be able to draw a straight line amongst these points, so that there should be no unequal preponderance of points on either side of the line throughout its length. I have traced the curves for several of the observers, and find that such is not the case. To test the point I have proceeded as follows:—

(1) All the values of excess of observed over tabular diameters have been reduced to an uniform standard with the assumed value of 11″·10 at the mean distance.
(2) Corrections for personal equations have been applied from the Tables (1) and (2).
(3) No observations have been employed, unless the same observer has made observations when the diameter was very small, near the mean, and large. This avoids any errors creeping into the work from imperfect correction for personal error.

The measures were then divided into groups with tabular diameters:—

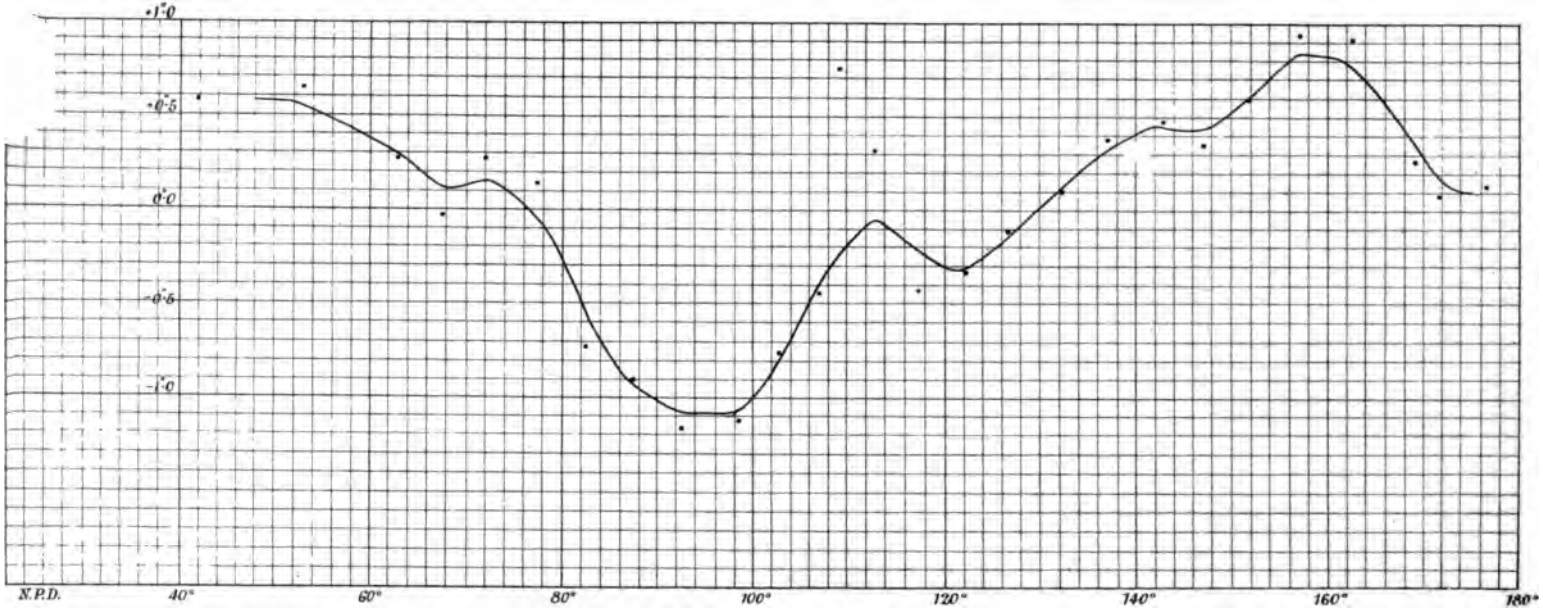
5″–7″; 7″–9″; 9″–12″; 12″–15″; 15″–17″;
17″–19″; 19″–22″; 22″–25″; above 25″.

The following table gives the results:—

Mean Factor.	No. of Observations.	Assumed Diameter at Earth's Mean Distance = 9″·71	= 10″·73	= 11″·10
7·48	5	+2·94	+2·24	+2·00
9·11	25	+2·39	+1·54	+1·24
12·21	49	+2·15	+1·01	+0·61
15·32	68	+2·05	+0·62	+0·12
17·80	80	+2·25	+0·60	+0·01
20·85	28	+1·76	−0·18	−0·37
23·21	35	+2·36	+0·21	−0·56
24·43	18	+2·15	−0·11	−0·92
27·33	11	+2·50	−0·04	−0·94

If these points be laid down to scale, the break of continuity in passing from the smaller measures is distinctly seen. It will be found that the straight line represented by Mr. Downing's solu-

COMPARISON OF THE NORTH POLAR DISTANCES OF THE CAPE CATALOGUES, FOR 1860 AND FOR 1840.



Spottiswoode & Co. Lith. London

The refractions used in the reductions were those of Bessel's *Tabulæ Regiomontanæ*.

The division errors of the mural Circles do not appear to have been determined. The flexure was considered insensible.

The places given in the Catalogue have been corrected for proper motion in only a few instances; in the present investigation a correction for proper motion has been applied to the Catalogue place whenever it became significant.

The particulars given above have been extracted from the Introduction to the Catalogue written by Mr. Stone; as the work is not yet distributed to the astronomical public, it appeared desirable to reproduce them in this place.

The North Polar Distances of this Catalogue have first been compared with those of the Cape Catalogue for 1860. There are 398 stars available for the comparison, after rejecting those whose places in either Catalogue depend on a single observation. The N.P.D.'s of these, as given in the 1860 Catalogue, were brought back to the epoch 1840 by applying the precessions, secular variations, and generally the proper motions of the 1860 Catalogue. In some instances these proper motions appeared to be erroneous, in which cases what was considered a more correct value was used instead of that given in the Catalogue. The differences between the N.P.D.'s of the 1860 Catalogue thus brought back and those of the 1840 Catalogue were then taken, and having been arranged in order of N.P.D., were combined in groups extending over 5° of N.P.D.; the means of the differences for each group being considered as the difference between the Catalogues for the mean N.P.D. of the group. These mean differences were then laid down as points on cross-ruled paper, and a curve (*see* diagram) drawn through them, which may be taken to represent the systematic differences of the N.P.D.'s of the Catalogues.

The differences as computed and as read off from the curve are as follows:—

N.P.D.	Number of Stars.	1860−1840 Means.	1860−1840 Curve.
° ′		″	″
41 57	3	+ ·57	+ ·57
53 7	3	+ ·66	+ ·55
62 16	12	+ ·28	+ ·30
67 43	22	− ·04	+ ·12
72 3	8	+ ·29	+ ·17
77 49	6	+ ·15	− ·04
82 34	14	− ·72	− ·55
87 3	8	− ·90	− ·92
92 11	4	−1·16	−1·09
98 4	13	−1·13	−1·06
103 0	8	− ·78	− ·79
107 29	15	− ·43	− ·34
112 50	18	+ ·32	− ·06
117 36	40	− ·42	− ·22
122 33	34	− ·35	− ·31
127 16	27	− ·11	− ·12
132 21	28	+ ·11	+ ·13
137 20	24	+ ·39	+ ·35
142 50	19	+ ·48	+ ·43
147 29	22	+ ·36	+ ·45
152 19	19	+ ·60	+ ·63
157 16	21	+ ·96	+ ·86
162 55	3	+ ·91	+ ·77
168 30	9	+ ·27	+ ·38
171 56	12	+ ·07	+ ·14
176 58	6	+ ·14	+ ·14

The following table gives the differences for every 4° of N.P.D. as read off from the curve, and also the comparisons with the Greenwich Catalogues for 1860 and for 1840, which are deduced from the table given at p. 172 of Mr. Christie's paper in vol. xlv. of the *Memoirs*.

N.P.D.	Cape 1860 *minus* Cape 1840.	Greenwich 1860 *minus* Cape 1840.	Greenwich 1840 *minus* Cape 1840.
°	"	"	"
48	+ ·57	+ 1·16	+ 1·03
52	+ ·55	+ 1·12	+ ·88
56	+ ·48	+ 1·02	+ ·64
60	+ ·38	+ ·89	+ ·49
64	+ ·25	+ ·73	+ ·35
68	+ ·13	+ ·59	+ ·27
72	+ ·17	+ ·62	+ ·43
76	+ ·01	+ ·42	+ ·42
80	− ·28	+ ·07	+ ·20
84	− ·69	− ·38	− ·09
88	− ·96	− ·62	− ·24
92	− 1·08	− ·71	− ·27
96	− 1·08	− ·67	− ·17
100	− ·99	− ·58	− ·09
104	− ·70	− ·37	+ ·05
108	− ·30	·00	+ ·23
112	− ·07	+ ·19	+ ·12
116	− ·16	+ ·04	+ ·15
120	− ·29	− ·16	+ ·41
122	− ·32	− ·19	+ 2·08
124	− ·27	− ·09	+ 3·93
128	− ·09		
132	+ ·11		
136	+ ·29		
140	+ ·41		
144	+ ·44		
148	+ ·47		
152	+ ·60		
156	+ ·81		
160	+ ·82		
164	+ ·70		
168	+ ·42		
172	+ ·13		

Comparisons with the other Greenwich Catalogues and with the Melbourne Catalogue can be found at once from the table in Mr. Christie's paper referred to above.

It should be remarked that the Cape and Greenwich Catalogues for 1840 give quite independent results to the Catalogues for the epoch 1860, as different instruments were in use at both Observatories at these dates.

A direct comparison has also been made between the Cape and Greenwich Catalogues for 1840, with results substantially agreeing with those given above. The number of stars used is 281, and proceeding generally as before the computation gives:—

Z.D. at Cape.	N.P.D.	Number of Stars.	Greenwich − Cape 1840 Means.	Smoothed Means.
	° ′		″	″
87	37 6	5	+5·59	+5·59
83¼	40 49	6	+2·86	+2·86
77½	46 26	18	+1·70	+1·70
73¼	50 45	6	+0·50	+ ·86
65¼	58 44	3	+ ·76	+ ·63
61½	62 35	13	+ ·49	+ ·50
56¼	67 49	26	+ ·23	+ ·35
51½	72 40	14	+ ·43	+ ·42
46¾	77 11	7	+ ·57	+ ·45
41½	82 24	17	+ ·23	+ ·16
Z.D. at Greenwich.				
48½	87 1	11	− ·40	− ·16
53½	92 4	6	+ ·01	− ·13
60	98 21	17	− ·12	+ ·02
64¼	102 47	13	+ ·29	+ ·15
69	107 22	24	+ ·12	+ ·28
74¾	113 15	24	+ ·55	+ ·48
78½	117 49	36	+ ·69	+ ·69
84	122 30	13	+1·56	+1·56
86¾	125 14	22	+2·81	+2·81

The smoothed means have been obtained by taking the means of every consecutive pair, omitting the two extreme values at each end of the series, and the means of these again; being in fact the same process as has been adopted in drawing the curve for comparison of the two Cape Catalogues.

The most noticeable feature in the comparison of the Cape Catalogues is the large difference in the neighbourhood of N.P.D. 95°; this is not to be attributed to large actual error in either of the Catalogues, as the comparison with Greenwich seems to show that it is about equally divided between them.

With regard to the question of astronomical refraction, the evidence afforded by this investigation, as far as it goes, seems to confirm the results deduced from the discussion of the N.P.D.'s of the Cape 1860 and Melbourne 1870 Catalogues—viz., that down to about 80° Z.D. the Greenwich observations are satisfactorily represented by the refractions of the *Tabulæ Regiomontanæ*, and that below that limit some such corrections as those proposed by Mr. Main are required. Thus, in the table above, the correction at 84° Z.D. is about $-1''$, and that at $86\frac{3}{4}°$ is about $-2''{\cdot}5$, and the discordances are then reduced to $+{\cdot}56$ and $+{\cdot}31$ respectively. At the same time it will be remarked that the Cape observations near the north horizon appear to require refractions diminished in a larger proportion, and corrections commencing at a less zenith distance than is the case at Greenwich. On this point, however, further investigation is desirable, the materials for which are now, we may hope, rapidly accumulating.

The comparison of N.P.D.'s of principal "Nautical Almanac" stars in the Greenwich and Cape Catalogues for 1840, given in the Introduction to the latter, has been utilised, for the stars situated between N.P.D. 60° and 118°, for the purpose of ascertaining whether there is any outstanding correction depending on the meteorological elements. For this purpose these stars have been combined in groups extending over six hours of R.A., with the result:—

R.A.	0^h–6^h	6^h–12^h	12^h–18^h	18^h–24^h
Greenwich – Cape	$-''{\cdot}02$	$+''{\cdot}08$	$+''{\cdot}53$	$+''{\cdot}22$.

If $+''{\cdot}10$ be taken as the systematic difference between the Catalogues for the limits of N.P.D. under consideration, we have—

R.A.	0^h–6^h	6^h–12^h	12^h–18^h	18^h–24^h
Greenwich – Cape	$-''{\cdot}12$	$-''{\cdot}02$	$+''{\cdot}43$	$+''{\cdot}12$.

The similarity of this result to that given by Mr. Stone as deduced from his own observations (*M. N.* vol. xl. p. 70) is very striking, and remarking the large relative discordance in the third group—that is, for observations made during May, June and July—tends to confirm his opinion that the complete reversion of the seasons at the northern and southern Observatories is not quite accurately allowed for in the refraction tables.

On the Apparent Conjunctions of the Satellites of Uranus with each other, 1881. By A. Marth, Esq.

When the apparent orbits of the satellites are flat ellipses, as they are at present, the satellites will not rarely pass one another at a short distance. Usually they will come to conjunction by encountering one another while moving in opposite directions

on different sides of the great axes of their orbits, but sometimes they will overtake one another while moving in the same direction. When two satellites are at or near conjunction, measurements of their position-angles in reference to the planet's centre cannot well be made with confidence, as the observer's judgment will not be unaffected, but the observation of their conjunction in distance, or the fixing of the time when they are at exactly the same distance from the planet, can be made with nicety, even without micrometer, and such observations will furnish very valuable tests for the theories of the satellites. I have therefore considered it worth while to make the present addition to the Ephemeris published on pages 67–72. During the interval of time over which the Ephemeris extends, some 160 conjunctions of satellites occur, the circumstances of which had to be examined. As I have not yet learnt how close to the planet the satellites can be seen in some of the most powerful telescopes, the limit of about 10″, which I have adopted, may perhaps be too exclusive. Disregarding all those conjunctions which take place within this limit of distance from the planet's centre, and those which on account of daylight and other hindrances offer no fair prospect of being observable in Europe or America, I give for the rest, in the following list, the position-angles and distances of the satellites for the nearest preceding and following even hour G.M.T., so that the circumstances of each conjunction may be seen at a glance:—

			Ariel.		*Umbriel.*		*Titania.*		*Oberon.*	
	1881.	G.M.T. h	Pos. °	Dist. ″	Pos. °	Dist. ″	Pos. °	Dist. ′	Pos. °	Dist. ″
Febr.	18	8	—		200·1	11·4	187·8	16·0	—	
		10	—		198·8	13·6	186·6	14·2	—	
		12	—		197·9	15·5	185·2	12·3	—	
	20	12	—		19·9	13·9	—		6·7	19·4
		14	—		17·9	15·8	—		5·8	17·8
		16	—		17·1	17·4	—		4·7	16·1
	27	10	197·1	12·8	—		—		183·6	15·8
		12	196·0	14·2	—		—		182·1	14·1
		14	195·2	15·1	—		—		180·2	12·4
March	3	8	15·5	14·9	—		7·5	17·7	—	
		10	14·6	15·3	—		6·4	15·8	—	
		12	13·8	15·1	—		4·8	13·7	—	
	4	16·0	194·7	15·3	—		203·1	15·2	—	
		16·6	194·5	15·3	—		202·7	15·7	—	
	7	10	192·2	13·3	195·5	20·7	189·6	22·9	210·6	12·1
		12	190·7	11·5	195·0	21·2	188·9	21·3	208·4	13·8
		14	188·8	9·2	194·4	21·3	188·2	19·6	206·7	15·5
		16	—		193·9	21·1	187·3	17·9	205·4	17·1

			Ariel.		*Umbriel.*		*Titania.*		*Oberon.*	
		G.M.T.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
	1881.	h	°	″	°	″	°	″	°	″
March		18	—		193·3	20·6	186·2	16·1	204·2	18·8
		20	—		192·7	19·7	184·8	14·2	203·3	20·4
	18	18	—		—		16·3	32·3	10·2	34·8
		20	—		—		16·0	33·0	9·7	33·3
	26	8	11·2	13·0	—		27·4	12·9	—	
		10	9·7	11·2	—		25·4	14 8	—	
		12	—		12·0	19·3	23·9	16·6	—	
		14	—		11·2	18·0	22·7	18·4	—	
	27	14	191·5	13·2	203·8	10·1	—		—	
		16	189·9	11·4	201·5	12·3	—		—	
		18	187·5	9·1	199·8	14·4	—		—	
	28	14	—		—		12·0	31·7	20·4	30·2
		16	—		—		11·7	30·8	19·9	31·5
April	2	8	20·4	10·0	—		—		352·1	11·0
		10	18·3	11·6	—		—		347·9	9·4
	7	8	22·0	8·8	—		1·4	13.6	—	
		10	19·4	11·1	—		359·0	11·8	—	
		12	17·6	13·0	—		355·6	9 9	—	
	8	14	—		—		204·1	17·0	183·0	20·4
		16	—		—		202·8	18·8	181·8	18·8
	14	10	—		—		14·2	34·4	9·5	35·2
		12	—		—		13·9	34·3	9·1	34·0
	15	10	—		17·3	14·5	9·2	25·8	1·1	18·4
		12	—		16·6	16·3	8·6	24·4	359·6	16·8
		14	—		16·1	17·8	7·9	22·9	357·8	15·2
		16	—		15·6	19·1	7·1	21·4	355·5	13·6
		18	—		15·2	20·0	6·1	19·7	352·7	12·0
	18	16	202·8	8·4	185·8	11·8	—		—	
		18	199·9	10·8	182·9	9·6	—		—	

On January 23 and 24 *Ariel* and *Titania*, moving in the same direction, will remain near one another; on January 27 and 28, *Umbriel* and *Titania*; on March 3 and 4, *Ariel* and *Titania*; on April 9, *Ariel* and *Oberon*; on April 25, *Umbriel* and *Titania*; on May 19 and 20, *Ariel* and *Oberon*.

I need not hint that when the two faint satellites or the two brighter satellites are at equal distances from the planet and near each other, the conditions are most favourable for comparing their brightness.

In March next a century will have elapsed since the dis-

covery of *Uranus* by Herschel. What is the present state of the group of stars which Herschel represents on Tab. xxv. p. 500 of vol. lxxi. of the "Philosophical Transactions"? From March 13 to April 19, 1781, Herschel determined the places of *Uranus* by measuring its distances and position-angles in reference to six stars, $a, \beta, \gamma, \delta, \epsilon, \zeta$. Of these stars of comparison, only a and ϵ, so far as I know, have been observed with meridian instruments—a, 1782, February 5 and 7, at Greenwich, and about 1840 by Rümker; and ϵ, by Rümker, in whose catalogue the places of three other stars of Herschel's figure are also found. The approximate positions of these stars for 1855, as given in the "Bonner Durchmusterung," and the more accurate positions for 1881·0, as deduced from Rümker's places for 1836, with the Pulkova precession constants, are the following:—

		1855.			1881.		
		a	δ		a	δ	
	m	h m s	° ′		h m s	° ′ ″	
a	9·0	5 40 30·4	+23 34·0		5 42 6·06	+23 35 6·7	
	9·0	40 52·8	38·9				
β	9·4	41 11·9	37·0				
γ	9·5	41 37·2	36·5				
δ wanting.							
η	8·7	41 43·3	25·9		5 43 17·82	23 26 6·2	
	9·1	42 20 8	49·6		5 43 55·71	23 50 15·7	
θ	8·8	42 26·3	52·8		5 44 0·99	23 53 30 8	
ϵ	8·8	44 6·1	34·0		5 45 40·68	+23 34 46·8	
ζ	9·4	5 44 17·3	+23 38·7				

Will not observers determine the places of the stars visible between the limits $5^h\ 42^m$ and $5^h\ 46^m$ in Right Ascension, and $23°\ 25'$ and $23°\ 55'$ in Declination, and make a map of the region? Herschel's sketch was made with a 7-inch reflector, so that no more powerful telescope would be required.

Observations of Comet e 1880 (*Swift*), *made at Dun Echt Observatory with the Filar Micrometer of the* 15·06 *inch Refractor.*

(*Communicated by the Earl of Crawford.*)

	Date.	Dun Echt Mean Time.	$\sigma - *$ $\Delta\alpha$	$\Delta\delta$	$\sigma\alpha$	$\sigma\delta$	Observer.
		h m s	m s	′ ″	h m s	° ′ ″	
a	1880, Nov. 7	15 30 19	+ 0 6·87	− 3 5·9	22 45 53·83	+42 33 43·3	J. G. L.
b	9	6 59 37	+ 4 31·27		22 57 58·10		,,
	9	7 27 1		− 8 19·4		+44 30 41·4	,,
c	10	6 10 29	+ 0 37·04	− 8 24·7	23 5 47·14	+45 35 54·2	R. C.
d	12	6 59 14	+ 0 4·89	+ 7 41·1	23 24 2·89	+47 51 33·2	,,
e	19	9 32 42	+ 0 10·24		0 48 52·03		J. G. L.
		10 14 6		+ 6 39·0		+53 50 35·3	,,
f		11 41 17	− 0 36·45		0 50 3·71		,,
		11 56 37		+ 7 10·0		+53 52 38·9	,,
g	20	5 58 26	+ 0 29·62	− 7 16·4	1 0 53·66	+54 13 12·1	,,
h	24	6 42 43	+ 2 11·58		1 59 15·03		,,
		7 6 59		−10 45·1		+54 52 42·0	,,
i	24	6 43 25	− 8 2·23				,,
		7 6 59		− 9 57·8			,,

	Date.	Dun Echt Mean Time.	δ − * Δα	Δδ	*δ	δδ	Observer.
		h m s	m s	′ ″	h m s	° ′ ″	
k	Nov. 25	7 11 40	+ 0 43·29	− 8 3·8	2 13 31·85	+54 43 49·2	J. G. L.
l	25	7 38 28	+ 1 7·07	− 3 26·7	2 13 47·05	+54 43 33·7	,,
m	27	7 54 36	− 1 16·92	− 4 17·4	2 41 3·88	+54 6 2·3	,,
n	29	8 32 2		+ 2 35·0		+53 5 7·7	,,
		8 33 34	+10 5·06		3 6 19·23		,,
o	Dec. 1	13 0 37		− 0 0·5		+51 39 41·4	,,
		13 26 8	+ 7 2·04		3 30 46·35		,,
p	6	12 55 10	+ 0 0·22	− 1 14·4			,,
q	8	8 25 48	− 2 3·78		4 27 25·36		,,
		8 51 2		+ 9 51·8		+46 9 25·2	,,
r	9	12 48 10		− 5 48·3		+45 10 39·8	,,
		13 11 8	− 4 29·36		4 34 48·98		,,
s	1881, Jan. 4	12 14 11	+ 0 3·16	−3 37·4	5 50 45·39	+29 20 9·4	,,
t	6	13 38 36	+ 0 8·45	−0 46·4	5 53 42·95	+28 35 31·4	,,

Adopted Mean Places of Comparison Stars for 1880.

	h m s	Red. s	δ	Red.	Authorities.
a	22 45 43·12	+3·84	+42° 36′ 10″·5	+38″·7	Lalande (Asten) 44726–7.
b	22 53 22·93	+3·90	+44 38 21·5	+39·3	½ (Bonn Cat. + 44°4293 + B.W. xxii. 1199).
c	23 5 6·06	+4·04	+45 43 39·3	+39·6	Arg. Oe. 25230.
d	23 23 53·71	+4·29	+47 43 12·2	+39·9	Arg. Oe. 25644.
e	0 48 36·08	+5·71	+53 43 18·2	+38·1	Rümker, N. F. 393.
f	0 50 34·42	+5·74	+53 44 50·9	+38 0	Bonn Cat. + 53°184.
g					μ Cassiop., "Conn. des Temps."
h	1 56 56·61	+6 84	+55 2 54·2	+32·9	Lalande (Asten.) 3757–8.
i	2 7 13·4	+6·98	+55 1·9	+31·8	D. M. + 54°500.
k	2 12 41·52	+7·04	+54 51 21·7	+31·3	Arg. Oe. 2642.
l	2 12 32·95	+7·03	+54 46 29·1	+31·3	Arg. Oe. 2638.
m	2 42 13·45	+7·35	+54 9 51·8	+27·9	Arg. Oe. 3201.
n					γ Persei, Auwers, "Fundamental Catalogue."
o	3 23 36·78	+7·53	+51 39 19·2	+22·7	Bonn Cat. + 51°744.
p	4 14 40·4	+7·54	+47 43·1	+15·3	D. M. + 47°985.
q	4 29 21·67	+7·47	+45 59 20·2	+13·2	Arg. Oe. 4980.
r	4 39 10·89	+7·45	+45 16 16·5	+11·6	Arg. Oe. 5159.

Adopted Mean Places of Comparison Stars for 1881.

s	5 50 39·33	+2·90	+29 23 46·5	+0·3	Observed with the Dun Echt Transit Circle.
t	5 53 31·60	+2·90	+28 36 17·8	+0·0	" "

Remarks.

Nov. 7. At $11^h\ 10^m$ Dun Echt M. T., this comet was independently found by Mr. J. G. Lohse. Owing to bad weather no exact observation could be secured until $15^h\ 30^m$. Very soon after the last measure, the comet disappeared in the rays of the 5·0 mag. star D. M. +42°4521 over which it must have subsequently passed; in the meantime the approaching daylight prevented further observations.

Nov. 10. To the original observation of the comparison star in Bonn Obs. I. p. 94, there is a note that "die Fäden stimmen nicht; sie geben 52″·78 und 53″·34." Sky very hazy; the last measure of 4 marked very doubtful, but as they all agree fairly, the mean has been taken. The result differs from that in Dun Echt Circular, No. 10, and *Ast. Nach.* vol. 98, p. 325, by $-0^s{\cdot}19$ and $+3''{\cdot}0$. The result here given is the right one derived from the mean of the computed $\Delta\alpha$ and $\Delta\delta$. That previously given was obtained by taking the mean of the position angles and distances, a method which may lead to considerable errors when the described angle is large.

Nov. 16, 17, and 18. The comet could not be found owing to strong moonlight.

Nov. 19. The comet was fairly well seen, although the Moon was several degrees above the horizon. The condensation in the middle was tolerably well defined and could easily be measured. The comet did not seem to be perfectly round, but rather a little elongated on the following side.

Nov. 20. Comet perfectly round, considerably brighter in the middle, but without a real nucleus.

Nov. 24. To-day the comet is apparently brighter and more extended than on Nov. 20. Although very diffused and ill-defined it could be easily measured when the sky was perfectly clear.

Nov. 25. Measured with power 122. With 229 the texture of the comet is uneven as compared with a nebula. It has no nucleus, although an area in the centre 20″ in diameter is brighter than the rest. Obviously it cannot bear high powers, for it is apparently brighter and larger in the finder (Apert. 3¾ in.) than in the main telescope. There is no appreciable deviation from the circular form.

Nov. 27. The comet, though decidedly somewhat brighter than on the 25th, remains round and hazy as before.

Dec. 1. No change since Nov. 29. In the 6·06 in. Simms refractor, with a large Kellner eye-piece, the comet appears very large and tolerably bright, but without a nucleus. The diameter is 10′·5, being equal to the distance between D. M. +51°757 and 760. The coma is so faint as only to be visible with very low powers, hence the remarkable result that its *apparent* size decreases with increased magnifying power. Thus, in the 15·06 in. refractor it is seen as a small faintish comet; in the finder it is already considerably larger and brighter, while in the 6·06 in. Simms it is quite a striking, though not very bright object.

Dec. 8. Clouds troublesome.

Dec. 9. Measures easy, the comet being still tolerably bright, considering the already strong moonlight.

1881, Jan. 4. The comet is exceedingly faint and very difficult to measure, as the smallest star involved renders it almost impossible to say where the centre is. The condensation in the middle is very insignificant.

Jan. 6. The comet is so exceedingly faint as only to be recognizable in a perfectly dark field; the measures are in consequence very difficult, All the measures were made with power 122, except on Jan. 6, when power 229 was also used. Each result is based on at least four comparisons. Observers, Ralph Copeland and J. G. Lohse.

Dun Echt Observatory,
1881, *Jan.* 13.

Further Researches on the Annual Parallax of 61 Cygni. By Professor Robert S. Ball, LL.D., F.R.S., Royal Astronomer of Ireland.

In the *Dunsink Observations*, Part III., pp. 16–39 ("Observatory," No. 17, vol. II., p. 137), I have discussed a series of measures of the difference between the declination of 61 A *Cygni* and a star of the 9–10 magnitude north following. The discussion of these observations gave an annual Parallax of $+0''{\cdot}4654 \pm 0''{\cdot}0497$. Shortly after this series was completed I commenced a new series, in which I employed the following star of 61 *Cygni*, often known as 61 B *Cygni*, instead of the preceding star which had been used in the previous researches. The same comparison star was employed, and the methods of observation and reduction were precisely the same. It is the object of the present paper to describe these observations, and to set forth the results to which they have led.

The series now under discussion was commenced upon September 18, 1878, and was concluded on October 2, 1879.

On each night of observation it was sought to take four complete measures of the difference of declinations. Each measure was derived from four readings of each of the screws in the direct position and four in the reversed position. The final result adopted for each night was the mean of the four separate measures.

To illustrate the nature of these observations and the method employed in their reduction, I here give a series of measures on a night, November 16, 1878, which I have selected at random from the entire series :—

Micrometric Measurements of the Difference of Declination of 61 B Cygni and Comparison Star on the Night of November 16, 1878.

Screw I.	Screw II.	Screw I.	Screw II.	Screw I.	Screw II.	Screw I.	Screw II.
63·698	40·627	55·783	32·139	64·318	40·047	55·599	32·446
63·753	40·598	55·857	32·134	64·163	40·177	55·562	32·506
63·791	40·537	55·902	32·063	64·175	40·237	55·501	32·549
63·823	40·537	55·921	32·074	64·097	40·257	55·483	32·633
55·639	32·356	64·217	40·184	55·906	32·143	63·557	40·863
55·666	32·353	64·215	40·142	55·697	32·293	63·293	41·103
55·732	32·268	64·202	40·113	55·716	32·303	63·267	41·088
55·753	32·262	64·213	40·095	55·708	32·348	63·154	41·228

Each of these eight columns has first to be reduced to seconds. This is accomplished by adding together the four larger numbers in each column, and subtracting from the result the sum of the four smaller numbers: *e.g.*, for the first column this result is 32·875. This has to be multiplied by one-eighth of the value of the micrometer Screw I., at the temperature of

the instrument (39°·9). The result thus found is 36″·292. In a similar manner the movement of Screw II. in the second column gives 37″·224. Thus the first set of measures on the night in question gives for the apparent total distance the result 73″·516. This must, however, receive a correction of +0″·026 on account of refraction, and of −0″·031 on account of reduction for Aberration, Precession, and Nutation,* so that the final result is 73″·511. In a similar manner we have for each of the other sets of observations on the night in question 73″·705, 73″·541, 73″·453. The mean of the whole four is 73″·552, which is accordingly taken as the final result of the observations for this one night.

In some cases the number of observations made was less than four, so it has been found convenient when estimating the weight attached to each night's work to employ simply the number which denotes the number of complete measures which have been obtained. Thus, for the night just considered, the weight is taken as 4.

In the following table is given the results of the observations on the several nights which are included in the present series:—

Mean Results of the Observations of the Difference in Declination between 61 *B Cygni and Comparison Star reduced for refraction and reduction.*

Date.	Diff.	Date.	Diff.	Date.	Diff.
1878.	″	1878.	″	1879.	″
Sept. 18	73·657	Nov. 17	73·609	Apr. 16	72·348
20	73·633	21	73·507	30	72·117
23	73·579	25	73·615	May 4	72·229
25	73·613	28	73·437	8	71·907
Oct. 23	73·520	Dec. 1	73·554	15	71·602
24	73·698	5	73·436	22	71·640
25	73·528	8	73·442	June 10	71·530
28	73·739	30	73·268	15	71·345
29	73·537	1879.		28	71·169
31	73·707	Jan. 8	73·400	July 26	70 853
Nov. 1	73·802	15	73·370	Sept. 17	70·424
8	73·633	18	73·325	Oct. 2	70·816
16	73·552	Apr. 10	72·512		

A glance at these results exhibits in a conspicuous manner the large proper motion of 61 B *Cygni* relatively to the star which has been chosen for comparison. To clear the observations from the grosser part of the effects of proper motion, it

* The methods of correcting such observations for refraction and reduction are similar to those already described in *Dunsink Observations*, Part III. pp. 19–22.

became necessary to adopt an approximate value of the relative annual proper motion in declination. The former series of observations had conclusively established the fact that the comparison star is not affected by any considerable proper motion. I therefore felt warranted in assuming Argelander's value of the absolutely proper motion of 61 B *Cyg.* in declination as approximately the relative proper motions. The value in question is +3′·016 (*Positiones Mediæ*, p. 27).

The mean epoch of the entire series of observations is 1879·069. The mean value of the observed differences of declination is 72″·85549. The assumed epoch is 1879·0, whence applying a correction of +·20865 for proper motion, we have as the adopted mean difference of declinations at the epoch 73″·06414.

The equations of condition are now formed in the usual manner.

The true mean distance at the epoch is assumed to be $73''\cdot06414-x$; the true value of the proper motion in declination is assumed to be $3''\cdot016+x'$; the correction to be applied to an observed difference of declinations for the parallax ϖ is $-[9\cdot92229]\cos(\odot-121^\circ\ 39')R\varpi$; the correction to be applied for a possible difference κ between the coefficients of aberration of the two stars is $-[9\cdot92229]\sin(\odot-121^\circ 39')\kappa$.

61 B Cygni.

Equations of Condition.				Weight.	Residual.
$-x+$·2868$x'+$·4907$\varpi+$·6785$\kappa-$·272 = 0				2	−·042
+·2814	+·4670	+·6947	−·280 = 0	2	−·060
+·2732	+·4301	+·7177	−·309 = 0	2	−·103
+·2675	+·4041	+·7325	−·258 = 0	2	−·062
+·1910	+·0177	+·8360	−·120 = 0	1	−·092
+·1882	+·0026	+·8362	+·066 = 0	4	+·087
+·1855	−·0111	+·8361	−·096 = 0	2	−·080
+·1773	−·0545	+·8344	+·140 = 0	2	+·135
+·1745	−·0697	+·8332	−·053 = 0	2	−·064
+·1692	−·0973	+·8304	+·133 = 0	4	+·110
+·1665	−·1116	+·8286	+·236 = 0	4	+·206
+·1473	−·2110	+·8085	+·125 = 0	2	+·048
+·1255	−·3198	+·7710	+·109 = 0	4	−·020
+·1227	−·3332	+·7652	+·175 = 0	4	+·040
+·1122	−·3838	+·7403	+·105 = 0	2	−·053
+·1009	−·4355	+·7101	+·247 = 0	4	+·063
+·0928	−·4718	+·6858	+·093 = 0	4	−·110
+·0843	−·5074	+·6589	+·236 = 0	4	+·016
+·0735	−·5511	+·6214	+·150 = 0	4	−·092

Equations of Condition.					Weight.	Residual.
+ ·0652	− ·5832	+ ·5902	+ ·181	= 0	4	− ·079
+ ·0054	− ·7587	+ ·3222	+ ·188	= 0	4	− ·167
− ·0200	− ·7996	+ ·1947	+ ·396	= 0	2	+ ·015
− ·0391	− ·8175	+ ·0922	+ ·424	= 0	4	+ ·029
− ·0473	− ·8214	+ ·0481	+ ·404	= 0	2	+ ·005
− ·2730	− ·1566	− ·8215	+ ·271	= 0	1	+ ·140
− ·2920	− ·0717	− ·8331	+ ·165	= 0	1	+ ·074
− ·3274	+ ·1275	− ·8265	+ ·040	= 0	2	+ ·047
− ·3383	+ ·1831	− ·8162	+ ·185	= 0	4	+ ·219
− ·3491	+ ·2382	− ·8022	− ·104	= 0	1	− ·043
− ·3685	+ ·3332	− ·7686	− ·351	= 0	2	− ·241
− ·3876	+ ·4226	− ·7246	− ·255	= 0	2	− ·098
− ·4394	+ ·6307	− ·5599	− ·209	= 0	2	+ ·082
− ·4530	+ ·6763	− ·5061	− ·353	= 0	1	− ·056
− ·4887	+ ·7717	− ·3507	− ·421	= 0	2	− ·065
− ·5657	+ ·8485	+ ·0297	− ·505	= 0	4	− ·079
− ·7100	+ ·5067	+ ·6669	− ·499	= 0	2	− ·175
− ·7516	+ ·3162	+ ·7741	+ ·019	= 0	2	+ ·266

From these I obtain the following normal equations:—

$$
\begin{aligned}
+97{\cdot}000\,x + 3{\cdot}4413\,x' + 10{\cdot}009\,\varpi - 36{\cdot}235\,\kappa - 3{\cdot}9766 &= 0\\
+\ 3{\cdot}4413\,x + 7{\cdot}5700\ - \ 5{\cdot}9279 + \ 9{\cdot}0188 + \ 2{\cdot}7095 &= 0\\
+10{\cdot}009\,x - 5{\cdot}9279 + 22{\cdot}683 - 11{\cdot}831 - 10{\cdot}504 &= 0\\
-36{\cdot}235\,x + 9{\cdot}0188 - 11\,831 + 44{\cdot}890 + \ 3{\cdot}9185 &= 0
\end{aligned}
$$

Solving these equations I obtain

$$
\begin{aligned}
x &= +0''{\cdot}0227 \pm 0''{\cdot}0194\\
x' &= -0{\cdot}0878 \pm 0{\cdot}0705\\
\varpi &= +0{\cdot}4676 \pm 0{\cdot}0321\\
\kappa &= +0{\cdot}0719 \pm 0{\cdot}0314
\end{aligned}
$$

These values when substituted in the equations give the residuals in the column following the equations of condition. The sum of the squares of the absolute terms is 6·172, while the sum of the squares of the residuals is but 1·223, the weights being of course taken into account in each case.

The probable error of one observation is ±0″·1298, whence the probable error of the mean of four observations, which form a complete series for one night, is ±0″·0649.

p Eridani.

Observer.	Epoch.	Mean Epoch.	Position.	Mean Position.	Distance.	Mean Dist.
Dunlop		1825·90		343·10		2·500
Herschel *	1834·85	1835·14	302·33	301·68	3·65	3·68
	1835·		302·30		3·65	
	1835·58		302·42		3·75	
Jacob	1845·87	1846·11	276·0	276·25	4·16	4·21
	1845·87		276·2		4·16	
	1845·88		275·8		—	
	1846·83		277·0		4·32	
Powell		1853·95		263·10		3·95
Jacob	1856·08	1856·03	262·11	259·68	4·92	4·62
	1856·09		260·06		4·63	
	1856·10		261·47		4·62	
	1857·90		257·18		4·38	
	1857·98		257·60		4·56	
	1858·02		258·93		4·55	
Powell		1861·34		253·37		4·86
Russell, Mean of 10 Obsvns.		1870·92		242·60		5·46
" Power 800, 10 Obsvns.	1878·16	1878·19	236·34	237·00	6·029	6·138
	1878·22		237·26		6·147	
" Power 800, 10 Obsvns.		1880·45		234·41		6·295

* 180° has been added to angle.

Sydney Observatory,
1880, *June* 17.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLI. FEBRUARY 11, 1881. No. 4.

J. R. HIND, Esq., F.R.S., President, in the Chair.

W. H. St. Quintin Gage, Esq., Eastbourne;
O. Theodor Olsen, Esq., 40 Cleethorpe Road, Grimsby; and
James George Vine, Esq., Rosslyn House, Bromfelde Road, Clapham, S.W.;
were balloted for, and duly elected Fellows of the Society.

REPORT OF THE COUNCIL TO THE SIXTY-FIRST ANNUAL GENERAL MEETING OF THE SOCIETY.

The following table shows the progress and present state of the Society:—

	Compounders	Annual Subscribers	Non-resident	Mathematical Society	Total Fellows	Associates	Patron	Grand Total
December 31, 1879 ...	214	368	4	5	591	43	1	635
Since elected	+6	+20	...	...	...	...	...	...
Deceased	−2	−7	−1	...	...	−2	...	...
Removals	+3	−3	...	...	...	...	...	...
Resigned	−1	−4	...	...	...	...	...	...
Expelled	...	−11	...	...	...	...	...	...
December 31, 1880 ...	220	363	3	5	591	41	1	633

Mr. Barrow's Account as Treasurer of the Royal

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance at Bankers', Dec. 31, 1879	217	7	0			
„ in hand of Secretary of Library Committee:						
On account of Turnor Fund	18	6	8			
„ Library Expenses ...	22	7	3			
				258	0	11
Dividend on £7,500 Consols	110	3	2			
„ £5,700 New 3 per cent. Stock ...	83	14	5			
„ £7,500 Consols	110	3	2			
„ £5,700 New 3 per cent. Stock ...	83	7	3			
				387	8	0
Received on account of Subscriptions:						
Arrears	173	5	0			
254 Contributions for 1880	533	8	0			
3 Contributions for 1881	6	6	0			
24 Admission Fees	50	8	0			
18 First Contributions	31	10	0			
				794	17	0
9 Composition Fees				189	0	0
Sales of Publications:						
At Society's Rooms, 1880	35	8	4			
At Williams & Norgate's, 1879	36	19	0			
				72	7	4
Due to Assistant Secretary on Petty Cash account, Dec. 31, 1880				5	4	0
				£1,706	17	3

Astronomical Society, from Dec. 31, 1879, to Dec. 31, 1880.

EXPENDITURE.

	£	s.	d.	£	s.	d.
Salaries:						
Editor of *Monthly Notices*	60	0	0			
Assistant Secretary	225	0	0			
				285	0	0
Income Tax and House Duty				9	3	9
Fire Insurance				7	16	6
Printing: Spottiswoode & Co.	331	12	0			
Hazell, Watson, & Viney	326	4	10			
				657	16	10
Lithography and Engraving				9	10	0
Turnor Fund: Books purchased during year ...				30	12	2
Library Expenses: Binding	40	8	10			
Library Catalogue	45	0	0			
				85	8	10
House expenses	31	16	9			
Wages	31	14	0			
Stamps and postage	57	1	4			
Carriage of books and parcels	5	9	10			
Stationery and office expenses	13	12	2			
Expenses of meetings	20	0	0			
Coals and gas	57	10	6			
Fittings in hall and stock room	29	15	2			
Sundry fittings and repairs	8	1	3			
Whitewashing, papering, &c.	17	2	0			
Sundries	9	2	5			
				281	5	5
Mrs. Jackson-Gwilt's annuity				8	19	0
Due to Assistant Secretary on Petty Cash account, Jan. 1, 1880				1	4	5
Balance at Bankers', Dec. 31, 1880	317	14	10			
„ in hand of Assistant Secretary on account of Turnor Fund	12	5	6			
				330	0	4
				£1,706	17	3

Examined and found correct, Jan. 6, 1881.

J. Rand Capron,
A. M. W. Downing.

Assets and present property of the Society, January 1, 1881:—

	£	s.	d.	£	s.	d.
Balance at Bankers', Dec. 31, 1880	317	14	10			
,, in hand of Assistant Secretary on account of Turnor Fund	12	5	6			
	330	0	4			
Less amount due to Assistant Secretary on Petty Cash account	5	4	0			
				324	16	4
Due on account of Subscriptions:						
1 Contribution of 4 years' standing ...	8	8	0			
14 ,, 3 ,, ...	88	4	0			
31 ,, 2 ,, ...	130	4	0			
49 ,, 1 ,, ...	102	18	0			
3 Admission Fees and First Contributions...	9	9	0			
Various amounts	8	8	0			
	347	11	0			
Less 3 Contributions paid in advance ...	6	6	0			
				341	5	0
Due for Publications	18	0	9			
,, from Williams & Norgate for sales during 1880	76	8	5			
				94	9	2

£7,500 Consols, including the Lee Fund (£300), the Turnor Fund (£450), and the Horrox Memorial Fund (£100).
£5,700 New 3 per cent. Stock, including Mrs. Jackson-Gwilt's gift (£300).
Astronomical and other MSS., Books, Prints, Instruments, &c.
Unsold Publications of the Society.
Five Gold Medals.

Report of the Auditors.

We, being two of the duly appointed Auditors, beg to lay before this General Meeting of the Royal Astronomical Society the following Report:—

1. We have examined the Treasurer's account, and an account of the assets and property of the Society, and have found and certified the same to be correct.

2. The receipts and expenditure for the past year are as stated in the Treasurer's account.

3. The cash in hand on December 31, 1880, including the balance at the bankers', amounted to 330*l.* 0*s.* 4*d.*

4. The funded property of the Society is the same as at the end of last year, and is in a satisfactory state.

5. We have laid on the table a list of the names of those Fellows who are now in arrear for sums due at the last Annual General Meeting, with the amount due against each Fellow's name.

J. RAND CAPRON,
A. M. W. DOWNING.

Stock in hand of volumes of the *Monthly Notices*:—

Vol.	At Society's Rooms	At Williams & Norgate's	Vol.	At Society's Rooms	At Williams & Norgate's
I.	78	1	XXII.	35	...
II.	78	2	XXIII.	32	...
III.	...	...	XXIV.	24	...
IV.	...	...	XXV.	7	...
V.	...	...	XXVI.	11	...
VI.	44	1	XXVII.	3	...
VII.	2	...	XXVIII.	76	...
VIII.	142	2	XXIX.	54	1
IX.	24	3	XXX.	68	4
X.	150	2	XXXI.	99	2
XI.	186	2	XXXII.	122	4
XII.	12	2	XXXIII.	106	3
XIII.	152	3	XXXIV.	84	5
XIV.	110	3	XXXV.	70	1
XV.	129	2	XXXVI.	39	1
XVI.	110	1	XXXVII.	46	2
XVII.	137	1	XXXVIII.	107	5
XVIII.	167	...	XXXIX.	113	2
XIX.	67	...	XL.	126	6
XX.	31	...	Index to *Monthly Notices*	597	...
XXI.	20	...			

In addition to the above volumes of the *Monthly Notices*, the Society has a considerable stock of separate numbers of nearly all the volumes. With the exception, however, of Vols. XXXVI. to XL., no complete volumes can be formed from the separate numbers in stock.

Stock in hand of volumes of the *Memoirs*:—

Vol.	At Society's Rooms	At Williams & Norgate's	Vol.	At Society's Rooms	At Williams & Norgate's
I. Part 1	7	...	XXIII.	157	1
I. Part 2	44	...	XXIV.	164	2
II. Part 1	57	...	XXV.	176	2
II. Part 2	23	...	XXVI.	180	2
III. Part 1	70	...	XXVII.	435	1
III. Part 2	89	...	XXVIII.	392	1
IV. Part 1	85	3	XXIX.	421	...
IV. Part 2	94	3	XXX.	170	...
V.	110	4	XXXI.	151	2
VI.	129	3	XXXII.	167	2
VII.	153	3	XXXIII.	173	1
VIII.	132	4	XXXIV.	172	7
IX.	140	4	XXXV.	119	4
X.	152	1	XXXVI. (with M.N.)	206	13
XI.	160	...	XXXVI. (without)	5	...
XII.	167	...	XXXVII. Part 1	357	8
XIII.	174	...	XXXVII. Part 2	305	6
XIV.	373	3	XXXVIII.	293	2
XV.	146	1	XXXIX. Part 1	266	4
XVI.	177	...	XXXIX. Part 2	275	5
XVII.	154	3	XL.	306	2
XVIII.	158	...	XLI.	493	5
XIX.	162	...	XLII.	271	6
XX.	161	2	XLIII.	297	3
XXI. Part 1	314	...	XLIV.	312	6
XXI. Part 2	99	...	XLV.	488	5
XXI. 1 & 2 (together)	67	1	Index to *Memoirs*	663	2
XXII.	162	1			

Instruments belonging to the Society.

No. 1. The *Harrison* clock.
„ 2. The *Owen* portable circles, by Jones.
„ 3. The *Beaufoy* circle.
„ 4. The *Beaufoy* transit instrument.
„ 5. The *Herschel* 7-foot telescope.

No. 6. The *Greig* universal instrument, by Reichenbach and Ertel. The transit telescope, by Ultzschneider and Fraunhofer, of Munich.
„ 7. The *Smeaton* equatoreal.
„ 8. The *Cavendish* apparatus.
„ 9. The 7-foot Gregorian telescope (late Mr. Shearman's).
„ 10. The variation transit instrument (late Mr. Shearman's).
„ 11. The universal quadrant, by Abraham Sharp.
„ 12. The *Fuller* theodolite.
„ 13. The standard scale, by Troughton and Simms.
„ 14. The *Beaufoy* clock, No. 1.
„ 15. The *Beaufoy* clock, No. 2.
„ 16. The *Wollaston* telescope.
„ 17. The *Lee* circle.
„ 18. The *Sharpe* reflecting circle.
„ 19. The *Brisbane* circle.
„ 20. The *Baker* universal equatoreal.
„ 21. The *Reade* transit.
„ 22. The *Matthew* equatoreal, by Cooke.
„ 23. The *Matthew* transit instrument.
„ 24. The *South* transit instrument.
„ 25. A quadrant, by Bird (formerly belonging to Captain Cook).
„ 26. A globe showing the precession of the equinoxes.
The *Sheepshanks* collection :—
„ 27. (1) 30-inch transit instrument, by Simms, with level and two iron stands.
„ 28. (2) 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.
„ 29. (3) $4\frac{6}{10}$-inch achromatic telescope, about 5 feet 6 inches focal length; finder; rack motion; double-image micrometer; two other micrometers; object-glass micrometer; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatoreal stand, clock movement.
„ 30. (4) $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.
„ 31. (5) $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.
„ 33. (7) 2-foot navy telescope.
„ 34. (8) A transit instrument of 45 inches focal length; with iron stand, and also Ys for fixing to stone piers; two axis levels.
„ 35. (9) Repeating theodolite, by Ertel, with folding tripod stand.

No. 36. (10) 8-inch pillar sextant, by Troughton, divided on platinum, with counterpoise stand and artificial horizon.

„ 37. (11) Portable zenith telescope and stand, $2\frac{3}{4}$-inch aperture and 26 inches focal length; 10-inch horizontal circle and 8-inch verticle circle, read to 10″ by two verniers to each circle.

„ 38. (12) 18-inch Borda repeating circle, by Troughton, $2\frac{1}{8}$-inch aperture and 24 inches focal length; the circles divided on silver, the horizontal circle being read by four verniers, and the vertical circle by three verniers, each to 10″.

„ 39. (13) 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms; circle divided on silver, reading to 10″; a 5-inch circle at eye-end reading to single minutes; horizontal circle 9 inches diameter in brass, reading to single minutes.

„ 40. (14) A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, reading to 10″; two sets of adjusting plates; tripod stand with enclosed telescope; heavy stand for theodolite; Y piece of level; two large and three small ground-glass bubbles divided; level collimator, object-glass $1\frac{5}{8}$-inch diameter and 16 inches focal length; micrometer eyepiece, comb, and wires; mercury bottle and trough.

„ 41. (15) Level collimator with object-glass $1\frac{7}{8}$-inch diameter and 16 inches focal length; stand, rider-level, and fittings.

„ 42. (16) 10-inch reflecting circle, by Troughton, reading by three verniers to 20″; counterpoise stand; artificial horizon with mercury; two tripod stands.

„ 43. (17) Hassler's reflecting circle, by Troughton, with counterpoise stand.

„ 44. (18) 6-inch reflecting and repeating circle, by Troughton and Simms, contained in three boxes, two of which form stands. Circle divided on silver, reading to single minutes; two inside arcs divided to single degrees, 150 degrees on each side; artificial horizon and mercury.

„ 45. (19) 5-inch reflecting and repeating circle, by Lenoir, of Paris.

„ 46. (20) Reflecting circle, by Jecker, of Paris, 11 inches in diameter, with one vernier reading to 15″.

„ 47. (21) Box sextant; reflecting plane and level.

„ 48. (22) Prismatic compass, by Troughton and Simms.

„ 49. (23) Mountain barometer.

„ 50. (24) Prismatic compass, by Thomas Jones, mounted with a cylindrical lens.

„ 51. (25) Ordinary $4\frac{1}{2}$-inch compass with needle.

No. 52. (26) Dipping needle, by Robinson.
„ 53. (27) Compass needle, mounted for variation.
„ 54. (28) Magnetic intensity needle, by Meyerstein, of Göttingen; a strongly fitted brass box with heavy magnet; filar suspension.
„ 55. (29) Box of magnetic apparatus.
„ 56. (30) Hassler's reflecting circle, by Troughton; a 10½-inch reflecting and repeating circle, with stand and counterpoise, divided on platinum with two movable and two fixed indices; four verniers reading to 10″.
„ 57. (31) Box sextant and glass plane artificial horizon, by Troughton and Simms.
„ 58. (32) Plane 2⅜-inch speculum, artificial horizon, and stand.
„ 59. (33) 2½-inch circular level horizon, by Dollond.
„ 60. (34) Artificial horizon, roof, and trough; the trough 8¼ by 4½ inches. Tripod stand.
„ 61. (35) Set of drawing instruments, consisting of 6-inch circular protractor and common protractor, T-square: one beam compass.
„ 62. (36) A pentagraph.
„ 63. (37) A noddy.
„ 64. (38) A small Galilean telescope with object-glass of rock crystal.
„ 65. (39) Five levels.
„ 66. (40) 18-inch celestial globe.
„ 67. (41) Varley stand for telescope.
„ 69. (43) Telescope, with the object-glass of rock crystal.
„ 70. Portable equatoreal stand.
„ 71. Portable altazimuth tripod.
„ 72. Four polarimeters.
„ 74. Registering spectroscope, with one large prism.
„ 76. Two five-prism direct-vision spectroscopes.
„ 78. 9¼-inch silvered-glass reflector and stand, by Browning.
„ 79. Spectroscope.
„ 80. A small box, containing three square-headed Nicol's prisms; two Babinet's compensators; two double-image prisms; three Savarts; one positive eyepiece, with Nicol's prism; one dark wedge.
„ 81. A back-staff, or Davis' quadrant.
„ 82. A nocturnal or star dial.
„ 83. An early non-achromatic telescope, of about 3 feet focal length, in oak tube, by Samuel Scatliffe, London.
„ 84. A Hollis observing chair.
„ 85. A double image micrometer, by Troughton and Simms.

No. 86. A 4½-inch Gregorian reflecting telescope, by Short, with altazimuth stand and 6-inch altitude and azimuth circles and two eyepieces.

„ 87. A 3¼-inch Gregorian reflecting telescope with wooden tripod stand.

„ 88. A pendulum with 5-foot brass suspension rod, working on knife edges, by Thomas Jones.

„ 89. A Rhabdological Abacus. A contrivance invented by Mr. H. Goodwyn, consisting of a box filled with compartments, in which are square rods covered with numbers, which can be arranged so as to facilitate the labour of multiplying high numbers.

„ 90. An Arabic celestial globe of bronze, not quite 6 inches in diameter.

„ 91. An astronomical time watchcase, by Professor Chevallier.

„ 92. A 2-foot protractor, with two moveable arms, and vernier.

„ 93. A beam compass, in box.

„ 94. A 2-foot navigation scale.

„ 95. Stand for testing measures of length.

„ 96. Artificial planet and star, for testing the measurement of a fixed distance at different position angles.

„ 97. A 12-cell Leclanché battery.

The following instruments have been presented to the Society by Mrs. Walter:—

No. 98. A 2 feet 6 inch navy telescope with object glass 2½ inches, by Cooke, with portable wooden tripod stand.

„ 99. A 12-inch transit instrument, by Fayrer & Son, with level and portable stand.

„ 100. A 9-inch transit instrument, with level and iron stand.

„ 101. A sun-dial, by G. Adams, London.

„ 102. A sun-dial, by Troughton.

„ 103. A sun-dial, by Casella.

„ 104. A sun-dial.

„ 105. A box sextant, by Troughton and Simms.

„ 106. An azimuth compass, by Schmalcalder, London.

„ 107. An azimuth compass, by C. Earle, Melbourne.

„ 108. An azimuth compass, by Negretti and Zambra.

„ 109. A dipleidoscope, by E. Dent.

„ 110. An Abney level, by Elliott.

„ 111. A pocket spectroscope, by Browning.

The following instruments are lent, during the pleasure of the Council, to the undermentioned persons:—

No. 4. The *Beaufoy* transit instrument, to the Observatory, Kingston, Canada.

„ 12. The *Fuller* theodolite, to the Director of the Sydney Observatory.

No. 22. The *Matthew* equatoreal, to Mr. Brett.
„ 23. The *Matthew* transit, to Captain Noble.
„ 74. Registering spectroscope, with prism, to Mr. Lecky.
„ 76. One 5-prism spectroscope, to Mr. Plummer.
„ 78. The 9¼-inch reflector, to Mr. Neison.

From the *Sheepshanks* collection:—

No. 29. (3) Parallel wire micrometer, to the Rev. W. C. Bruce.
„ 30. (4) 3¼-inch equatoreal and stand, to Mr. Sadler.
„ 31. (5) 2¾-inch telescope and stand, to Mr. Birt.
„ 34. (8) Transit instrument, to the Rev. Professor Pritchard.
„ 35. (9) Repeating theodolite, to the Sydney Observatory.
69. (43) Telescope, with rock-crystal object-glass, to Dr. Huggins.

The Gold Medal.

The Council have awarded the Society's Gold Medal to Professor Axel Möller, for his investigations of the motion of Faye's Comet. The President will lay before the Society the grounds upon which this award has been founded.

Publications of the Society.

Vol. XLV. of the *Memoirs* has been published during the past year. It contains the following papers:—

M. Souillart, 'Théorie analytique des mouvements des Satellites de Jupiter.'

W. H. M. Christie, 'On the Systematic Errors of the Greenwich North Polar Distances.'

Mr. Gill's Expedition for observing the Opposition of Mars.

Mr. Gill's account of his observations and reductions in connection with his expedition to Ascension to observe the Opposition of *Mars* in 1877 has been received by the Council, and the printing of it has been already commenced. It will appear in the *Memoirs*.

The Library.

During the year, the cataloguing has been steadily continued. About 6000 titles are now complete, and less than 3000 remain to be written. The library committee have drawn up a list of books, consisting of star atlases, works of reference, &c., which are not to be taken out of the library.

Schwabe's Sun-spot Observations.

In 1864, in response to a request from Dr. De la Rue and Prof. Balfour Stewart, Hofrath Schwabe allowed his valuable series of Sun-spot observations to be deposited in the Kew Observatory, and at the same time, in a letter printed in the *Monthly Notices*, vol. xxxvi. p. 298, desired that after his death they should be considered to be the property of this Society. These manuscripts have hitherto remained at Kew, where considerable use has been made of them in the reduction of the Kew solar photograms; but in the course of last year they have been transferred to the Society's library at the request of the Council.

The Transit of Venus, 1882.

At the request of the Treasury, a committee has been appointed by the Royal Society to advise the Government upon the steps which it is desirable to take in order to secure observations of the Transit of *Venus* across the Sun's disk 1882, December 6. The committee—which consists of the Astronomer Royal, the President of the Royal Society, the President of the Royal Astronomical Society, Professor J. C. Adams, the Earl of Crawford and Balcarres, Dr. De La Rue, Dr. Huggins, Professor H. J. S. Smith, Professor Stokes, and Mr. Stone—has already commenced its labours.

OBITUARY.

The Council regret that they have to record the loss by death of the following Fellows and Associates during the past year:—

Fellows:—C. Raganootha Chary.
Rev. R. S. Bower.
H. W. Buxton.
J. S. Eiffe.
William Gray.
W. A. Harris.
William Lassell.
Rev. R. C. Lumsden.
Captain J. Williams.

Associates:—Baron Dembowski.
Benjamin Peirce.
Prof. C. A. F. Peters.

CHINTAMANNY RAGOONATHA CHARY, the head assistant in the Madras Observatory, died on February 5, 1880. He was attached to the Observatory for a period of nearly forty years, and served in succession in every grade under Captain Jacob and the present astronomer, Mr. Pogson. He contributed three papers to the *Monthly Notices*, viz.: 'On the Determination of Personal Equa-

tion by Observations of the Projected Image of the Sun" (vol. xix. p. 337); "Occultations visible in the month of August, 1868, at Madras and along the Shadow-Path of the Total Eclipse of the Sun in India" (vol. xxviii. p. 193); "On the Total Eclipse of the Sun, on December 11th, 1871, as visible in the Madras Presidency" (vol. xxxi. p. 137). He was elected a Fellow of the Society on January 12, 1872.

In 1874 he published a pamphlet on the Transit of Venus, being induced to do so, as he states, by the fact that "although the class of phenomena to which the Transit of Venus belongs is mentioned in Hindu treatises on Astronomy, especially of the Sidhanta Siromani, yet the Sidhantis or Hindu astronomers are really not familiar with the nature of this particular occurrence, and cannot predict it with even a rough approach to accuracy, happening as it does at such strange and rare intervals." The English edition is in the form of a dialogue, and the author mentions that he had been accustomed for so many years to discuss astronomical facts and methods verbally with Hindu professors of the art that his sketch naturally assumed this shape. It is stated in the preface that in the Sanscrit, Canarese, Malayalum, and Maharathi versions it was found convenient to vary this arrangement. The library of the Society contains, besides the English edition, copies of the version in Hindustani and Telugu. Among the list of subscribers to the pamphlet are the Government of Madras, 1,100 copies; of Bombay, 900; of the N.W. provinces, 300; the Chief Commissioner, Central Provinces, 200; the Maharajah Holkar, 240; Education Department, Bombay, 200; besides eleven subscribers for 100 or more copies, eight for forty or more, &c.

At the end of the pamphlet Mr. Chary printed an address delivered by him "at Pacheappah's Hall, Madras, on April 13, 1874, to a large meeting of native gentlemen." The object was to obtain support for a work upon Astronomy which should embody the corrections, equations and formulæ established by European research, "together with what is proper to retain from our own works, and thus to construct a manual accessible to Hindu astronomers, and sufficient for all the purposes to which Astronomy is applied in our social and religious practices." The title of this work was to be "Jyotisha Chintamani," and it was to consist of two volumes of about 500 and 300 pages respectively. Mr. Chary states that for five years he has devoted his leisure to this task, and that he intends to take eighteen months' furlough shortly, and to devote the whole of his energies to bringing the work to a conclusion. After explaining the cost of printing the book, and asking for assistance to enable him to undertake the publication, he proceeds: "It is a matter of the deepest regret that I should be unable to carry out my design, so long the cherished object of my life, without invoking help from others. But you are all aware that my income has always been very slight, barely sufficient to provide for simple wants; an allowance of Rs. 100 per mensem after thirty-

three years' service leaves scanty margin for saving, and what little I have managed to put aside has been dispersed in meeting the charges which have had to be incurred in proceeding with the work so far. My health, too, is becoming weaker daily, and I cannot shut my eyes to the fact that my retirement from active work is not very distant."

He also suggested that a native Observatory should be founded by voluntary effort, and offered to present his own instruments as a nucleus of such an institution; after referring to the importance of co-operation in science, he adds: "In Europe, excluding Russia, there now exist fifty-four public and ten private Observatories spread over an area of less than two million square miles. In India with a surface of one and a half million miles we have but one, and that one wholly supported by the State . . . I recommend no more than that a modest but thorough place of instruction and study should be founded where theoretical knowledge can be united to actual practical work, and where the mathematician equally with the humble Sidhanti may find pleasure and profit. Such places exist in hundreds in Europe, but nowhere is the need for them greater than in India. Not much money, a little zeal, a little stedfastness of purpose, wed these to a regard for science, and soon would the metropolis of Southern India be graced with an Institution which would be an honour to the country."

In an obituary notice of Mr. Chary which appeared in the *Madras Mail* for February 7, 1880, he is spoken of as an unassuming and distinguished native gentleman, and it is stated that his strict honesty and ready skill as an observer, combined with accuracy and speed in computation, and a fair and useful amount of self-acquired mathematical knowledge, rendered him, until disabled by impaired health, invaluable in the Observatory, and that the chief share in the catalogue of stars in hand, with the Transit Circle, since 1862, comprising already over 38,000 separate observations, is due to his personal exertions. The notice concludes as follows:—"He was twice successfully engaged in observations of total eclipses of the Sun: on the first occasion in August 1868, at Vunpurthy, in the Nizam's dominions, in independent charge of a branch expedition for the purpose; and on the second, in December 1871, at Avenasky, in the Coimbatore District. He was the first and only native of India who has yet entered the lists as a discoverer of new celestial objects, having detected two new variable stars, *R Reticuli*, in 1867, and *V Cephei*, in 1878. As editor of the astronomical portion of the Asylum Press Almanac, and of a native calendar published on his own responsibility, he has been before the public for the past twelve years; and he latterly took great interest in delivering public lectures on Astronomy, with a view to enlighten his countrymen upon the subject, and to convince them of the absurdity of their notions in regard to celestial phenomena, by familiar explanations, in simple terms, of the true principles of

the science, as opposed to the ignorant superstitions and rough predictions of Hindu astrologers and empirics of the old school." It is probable that his projected book was not published; there is no copy of any portion of it in the Society's library, and the writer of the notice in the *Madras Mail* does not refer to it.

JAMES SWEETMAN EIFFE was the second son of Luke Eiffe, Esq., of Norman's Grove and Tirlstone, near Dublin, a gentleman of hunting celebrity in the past generation. He was born at his father's town residence in Rutland Square on December 25, 1804, and was intended for the army, an uncle offering him a commission; but a passion for horology overcoming every other consideration, he, soon after the completion of the full course of his collegiate education, and in opposition to the wishes of his family, directed his whole energies to the pursuit of his favourite science, and made everything subordinate to the one object of making the chronometer a perfect time-keeper. For his inventions for equalising the rates of chronometers in extremes of temperature, he received from the Government a reward, which, however, he only accepted as part payment of his claim. He also made various improvements in the astronomical clock. In 1842, an account of his improvements in chronometers was published by the Lords Commissioners of the Admiralty.

From a pecuniary point of view his efforts were unremunerative, and he exhausted his private resources and those of his wife in his experiments. He leaves a widow and an only son. He was elected a Fellow of this Society on April 8, 1842. He died at Ashford, Kent, on October 28, 1880.

JOHN GEORGE COCKBURN CURTIS GODSMAN, whose death was announced in last year's Report, was born on April 6, 1817. When five years old he accompanied his father, an officer in the Royal Navy, on a voyage to India. He remained about two years at Madras, and then returned to England, and was educated at the Royal Naval School at Greenwich. At the age of thirteen he entered the Royal Navy as a volunteer of the first class, and sailed to Mauritius, whence he returned in about three years. At the age of seventeen he was appointed to H.M.S. *Sulphur* as midshipman, and the principal part of the ten subsequent years was spent by him at sea. Upon his return to England, having quitted the Royal Navy, he was, in conjunction with Captain Wolfe and Lieutenant Beechy, directed by the Admiralty to make a survey of the Cove of Cork. Upon the completion of this survey he entered the Hydrographer's office. In 1845 he was selected by the Admiralty for the discharge of important duties in connection with the Tidal Harbours Commission. From 1846 to 1849 he was employed by the local authorities as well as by the Admiralty to conduct investigations upon the tidal phe-

nomena of the Mersey, Nene, and Dee. In May 1849 he surveyed the lower portion of the river Nene, in connection with the Admiralty inquiry into the Norfolk Estuary Bill of that year; and in April 1850 he reported to the Admiralty on the proposed Hartlepool West Harbour and Docks.

In 1852 he was at New York engaged in the Croton Aqueduct department. He then went to Spain, and for a considerable time worked under the Spanish Government in the irrigation of the 'Patrimonia Reale.' In 1861, as Admiralty Inspector, he held an inquiry on the proposed Nene Valley Drainage and Navigation Works. In 1864, he went to Turkey, and was employed on the Smyrna and Casaba Railway. His last work was in 1868, when he was requested to report upon the utilisation of the fresh-water supply of the Thorney river, a tributary to the Nene.

In 1877 he had a slight attack of paralysis, from which, however, he quickly recovered. On December 4, 1879, he was stricken with apoplexy in Westminster, and died on the following day.

He was elected a Fellow of this Society on January 10, 1845; he was also a member of the Institute of Civil Engineers.

He was known for the greater part of his life as Mr. John George Cockburn Curtis, the name of Godsman having been assumed by him only in recent years.

Henry Parr Hamilton, whose death was announced in last year's Report, was the son of Alexander Hamilton, physician, Professor in the University of Edinburgh, and was descended from an elder branch of the ducal family of the same name. He was born on April 3, 1794, and was educated at the High School, Edinburgh, under Dugald Stewart. He thence proceeded to Trinity College, Cambridge, having been entered on the side of Mr. Young. In the Admission Book of the college, under date October 21, 1811, he gives his native place as Blandfield, Midlothian, and the name of his master as Dr. Baird. He entered the college as a pensioner and was elected scholar in 1814. He graduated as ninth wrangler, in 1816, Whewell being second wrangler, and Richard Sheepshanks tenth wrangler, in the same year. He was elected Fellow of Trinity in 1818, at the same time as Connop Thirlwall and J. C. Hare, and held various college offices from 1819 to 1830: viz., Sub-Lecturer (*i.e.* examiner in the college examinations), 1820, 1821, 1823, 1825; Latin Lecturer, 1824; Head Lecturer (*i.e.* chief examiner), 1830; Junior Dean, 1823–25, Senior Dean, 1825–30. In 1830 he was presented by the Marquis of Ailesbury to the rectory of Wath, near Ripon, where he remained until 1850, when he was appointed Dean of Salisbury, in succession to the Very Rev. Francis Lear. He died at Salisbury on Feb. 7, 1880, aged 86. For the last twenty years of his life his health was very weak, and he seldom appeared in public. The restoration of the Cathedral was, however, a subject of great interest with him to the last. He may be said to

have been the originator of this work, which was the object of his constant solicitude, and towards which he contributed many thousands of pounds. His kindness of heart and his liberality are widely known; and even in the University he is still remembered for the amiable and courteous manner in which he discharged his duties.

On October 24, 1833, he married Eleanor, eldest daughter of Thomas Mason, Esq., of Copt Hewick Hall, Ripon, who survives him. His only child, Katharine Jane, is married to Sir Edward Hulse, Bart.

In 1826 he published *The Principles of Analytical Geometry designed for the Use of Students in the University*, a book of 320 pp. 8vo., in the Advertisement to which he states that "the work was undertaken with a view of illustrating the importance of Analytical Geometry as a method of investigation," and that his principal end will have been attained if it "shall be the means of attracting the attention of the student to a branch of science which is of extensive utility in the higher departments of mathematics;" and in 1828 he published *An Analytical System of Conic Sections designed for the Use of Students in the University* (pp. 189). In the preface to the latter he explains that the design of the work is to facilitate the study of Conic Sections "upon principles adapted to the present state of mathematical science." Referring to the former work (in which the discussion of the general equation of the second degree preceded that of the parabola, ellipse, and hyperbola), he states that "subsequent experience has taught him that this method of treating Conic Sections, although sanctioned by the practice of very eminent writers on the Continent, is too scientific, if he may use the expression, for elementary instruction." This objection he meets by adopting the focus and directrix definition of the Conic Sections (for which, he states, he is indebted to Boscovich*), and by deducing from it and discussing in separate chapters the equations of the parabola, ellipse, and hyperbola referred to their axes. This work went through five editions: the second was published in 1830, the fourth in 1838, and the fifth in 1843.

There was also a German translation: *H. P. Hamilton's System der Kegelschnitte analytisch dargestellt. Aus dem Englischen übersetzt von J. H. Beuchendorff* (Berlin, 1828). The articles "Analytical Geometry" and "Conic Sections" in the first volume of the *Encyclopædia Metropolitana*, were written by Hamilton; the date of these articles is probably about 1826. He was also the author of the following pamphlets, chiefly relat-

* The property of the focus, directrix, and determining ratio was known to Pappus. It was first brought fully to light by Sir I. Newton, and used as a definition by Boscovich. It seems to have been first introduced into a Cambridge text-book on Conics by T. Newton, in 1794, in his *Short Treatise on the Conic Sections*: see C. Taylor's *Ancient and Modern Geometry of Conics* (1881), p. lxxi.

ing to education:—*The Education of the Lower Classes* (1841); *A Sermon at Ripon at the triennial Visitation of the Bishop of Ripon, September* 3, 1844; *Practical Remarks on Popular Education in England and Wales* (1847); *The Church and the Education Question* (1848, and, second edition, 1855); *The Privy Council and the National Society* (1850).

Hamilton's work was not the first English treatise on Analytical Geometry, as Lardner's *Algebraic Geometry* was published three years earlier, in 1823, but it was the first Cambridge book, and the first which included Solid Geometry. The first edition of Wood's *Algebra*, which appeared in 1795, contained, as Part IV., a chapter of 30 pages "On the Application of Algebra to Geometry," in which are given the equations of the straight line, ellipse, cissoid, conchoid, the construction of equations, &c. This chapter remained unaltered in the ninth edition (1830), and until the publication of Hamilton's book seems to have formed the only introduction to Analytical Geometry existing in the University. The Senate House papers from 1800 to 1820 show that at the beginning of the century Analytical Geometry was always represented to some extent, though scarcely as an independent subject, most of the questions relating to areas, loci, &c., in which but little more than the mode of representation by means of ordinates and abscissæ was involved. In 1833, Peacock published (anonymously) his *Syllabus of Trigonometry, and the Application of Algebra to Geometry*, seventy pages of which are devoted to Analytical Geometry; there was a second edition in 1836. Hymers's *Conic Sections* appeared in 1837; it superseded Hamilton's in the University, and remained the standard work until the publication of Salmon's, Todhunter's, and other textbooks still in use. Hamilton's work, however, was used in schools for years after it had been replaced by Hymers's in the University. Hymers's *Analytical Geometry of Three Dimensions* was published in 1830. The other early English books on Analytical Geometry are *The Elements of Curves* (Anon., Oxford, 1828) and the treatises of Young (1830), Waud (1835), and Wright (1836).

It will thus be seen that the publication of Hamilton's works is of historic importance in connection with the studies of the University, and that they helped onward the great movement started by Herschel, Peacock, and Babbage, for the reform of English mathematics, which at length succeeded in placing Cambridge students in an equal position with those of other European countries. The history of the introduction of the Differential Calculus into the University in place of the Newtonian method of Fluxions is well known and need not be referred to further here; a short account will be found in the biographical notice of Peacock published in the *Monthly Notices*, vol. xix. pp. 125–128 (1859). One sentence from this account may, however, be quoted. Referring to Peacock's Syllabus of Trigonometry and Analytical Geometry, it is remarked that, "the reformers had kept their

eye too exclusively upon the Differential Calculus, of which this publication was a kind of confession." This seems to be true, and it is to be observed that, although great interest has been displayed in the history of the introduction of the Differential Calculus into the Cambridge examinations, but little attention has been devoted to the gradual growth of Analytical Geometry as a separate subject, which was taking place at about the same time. It is interesting to remark that the reasons which induced Hamilton to recur to the focus and directrix property as a definition still hold good; and that, although there is no doubt that, in a scientific development of the subject, the discussion of the general equation of the second degree should precede that of the particular cases, the subject is rendered much easier for the beginner if the parabola, ellipse, and hyperbola are considered separately immediately after the circle; and the method adopted by Hamilton in his *Conic Sections* has been generally followed in the multitude of text-books which have been published in recent years.

The Royal Astronomical Society had its origin in a meeting of fourteen gentlemen at Freemasons' Tavern on January 12, 1820, and was formally founded on February 8, 1820. Only two months later, on April 14, Hamilton was elected a Fellow, and at the time of his death he was the oldest Fellow of the Society. He thus connected the present time, not only with the revival of mathematics in England, but also with the foundation of the Society. Nearly all his contemporaries who were engaged in similar work near the beginning of the century, Herschel, Peacock, Babbage, Whewell, De Morgan, have passed away and their share in the movement has already been recorded in the pages of the *Monthly Notices*. It may be remarked, however, that the Society still numbers among its Fellows Mr. J. C. Ebden, who was sixth wrangler in Hamilton's year (1816), and was elected a Fellow of the Society only two months later than Hamilton, on June 9, 1820. Hamilton was elected a Fellow of the Royal Society on January 17, 1828; he was also a Fellow of the Royal Society of Edinburgh, and of the Geological Society. J. W. L. G.

William Augustus Harris was born at Bovey Tracey, in Devonshire, and was educated at Blundell's School, Tiverton. He obtained a Scholarship at Balliol College, Oxford, and was placed in the second class in the Final Examination in Law and Modern History. In 1869 he was called to the English bar, and in 1870 to the American bar. His constant trips to the United States in the years 1871–1877 greatly impaired his health, which completely broke down in 1878. He spent the earlier part of the winter of that year at Davos, and then proceeded to San Remo, whence he returned to England in May 1879 rather worse than better for his residence abroad. In the February following

he died of consumption at the early age of 33, in the midst of a promising career.

He was the author of several books, the last of which—*Harris's Mining Laws*, published in 1877—is a work of much use to the profession.

He was elected a Fellow of this Society on February 11, 1870, and was a member of the Eclipse Expedition to Sicily in that year.

WILLIAM LASSELL died at Maidenhead, October 5, 1880, aged 81 years. He was born at Bolton, Lancashire, June 18, 1799. He acquired the rudiments of education at a day school in his native town, during which period his father died, and he thence went for eighteen months to an academy at Rochdale.

In 1814 he entered a merchant's office at Liverpool, and there served a seven years' apprenticeship. He commenced business in Liverpool as a brewer about the year 1825, without, however, much taste or inclination for trade, and spent almost all his leisure time in his favourite pursuit of astronomy and the mechanics connected therewith.

Mr. Lassell possessed a great love and aptitude for mechanical invention, and for this reason "he belonged," to use the words of Sir John Herschel, "to that class of observers who have created their own instrumental means, who have felt their own wants, and supplied them in their own way." The qualities which enabled Mr. Lassell to do all this made him what he was. The work was the revelation of the man. He felt precisely where lay the difficulties and wants which met him in his work, because he was sensitive and sympathetic. He could deal successfully with these difficulties and supply these wants often in a masterly and original way, because he could think for himself cautiously yet boldly. He could work out his conceptions in new and difficult directions to a successful issue, because the constancy of his character showed itself here in concentration of thought and perseverance of action. These qualities, sensitive sympathy, wise prudence, constancy, were those which pre-eminently characterised him as a man and made him to those who knew him a friend of rare worth.

In the history of science Mr. Lassell's name will rank with those of Herschel and the late Lord Rosse in connection with that essentially British instrument, the reflecting telescope, whether we consider the genius and perseverance displayed in the construction of these instruments, or the important discoveries which have resulted from their use. About 1820 Mr. Lassell, then in his twenty-first year, began to construct reflecting telescopes for himself. It is perhaps to circumstances which he at the time considered unfavourable that science is indebted for much that Mr. Lassell has accomplished. At that time he did not possess sufficient means to enable him to purchase expensive instruments, and besides "his business avocations were such as

most men consider of an engrossing nature." The value to him in his subsequent work of the energy and power of resource which were in this way so strongly developed in his character at an early age, it is difficult rightly to appraise. His success with the first two instruments, which he attempted simultaneously (a Newtonian of seven inches' diameter and a Gregorian of the same size), encouraged him to make a Newtonian of nine inches' aperture. The instrument, which was erected in an Observatory at his residence near Liverpool, happily named Starfield, may be said to form an epoch in the history of the reflecting telescope, in consequence of the successful way in which Mr. Lassell, on a plan of his own, secured to it the inestimable advantages of the equatoreal movement. The several mirrors made for this instrument were of great excellence. The Observatory note-books of the late Mr. Dawes, which are in the writer's possession, bear record to the delicate tests for figure to which these mirrors were put on the occasion of the frequent visits of Mr. Dawes to his friend's Observatory. With this instrument Mr. Lassell worked diligently, and detected, without knowledge of its existence, the sixth star in the trapezium of the Nebula of *Orion*. This instrument is fully described in the twelfth volume of the *Memoirs* of the Royal Astronomical Society.

About the year 1844 Mr. Lassell conceived the bold idea of constructing a reflector of two feet aperture and twenty feet local length, to be mounted equatoreally on the same principle. He spared neither pains nor cost to make this instrument as perfect as possible, both optically and mechanically. As a preliminary step he visited the late Earl of Rosse at Birr Castle, and commenced the specula for the new instrument upon a machine similar in construction to that employed by that nobleman. After some months' work he was not satisfied with this apparatus, and was led in consequence to contrive a machine for imitating as closely as possible those motions of the hand by which he had been accustomed to produce perfect surfaces on smaller specula. "The essential difference of these constructions," to use the words of Sir George Airy, "as regards the movements of the grinder, is this: that in Lord Rosse's apparatus every stroke is very nearly straight, while in Mr. Lassell's apparatus there is no resemblance to a straight movement at any part of the stroke." This is not the place to describe the many new contrivances in the mode of support of the mirror, in the equatoreal mounting, in the polishing machine, and in the arrangements of the dome under which it was erected, which enabled Mr. Lassell to bring his telescope to a high degree of perfection. In this connection it should be noticed, to use Sir John Herschel's words, "that in Mr. Nasmyth he was fortunate to find a mechanist capable of executing in the highest perfection all his conceptions, and prepared by his own love of astronomy and practical acquaintance with astronomical observations, and with the construction of specula, to give them their full effect."

With this fine instrument he discovered the satellite of *Neptune*. This minute body was seen on October 10, 1846, but it was not until the next year that it could be satisfactorily followed and its existence fully confirmed. The superiority of the telescope and the vigilance and skill of the observer were further shown by the discovery in 1848, simultaneously with Professor Bond in America, of an eighth satellite of *Saturn*, of extreme minuteness, which was named *Hyperion*. In 1851, after long and careful search, he established the existence of two additional satellites of the planet *Uranus* (*Umbriel* and *Ariel*), interior to the two discovered by Sir William Herschel in 1787.

In the autumn of 1852 he took this twenty-foot telescope to Malta, and observed with it there through the winter. A very careful and detailed drawing of the Nebula of *Orion*, and drawings of several planetary nebulæ made at Malta, will be found in vol. xxxiii. of the *Memoirs* of the Royal Astronomical Society. With respect to the planets, his discoveries, to use his own words, "were rather negative than otherwise," for he was satisfied, that without great increase of optical power, no other satellite of *Neptune* could be detected. With regard to *Uranus* he says: "I am fully persuaded that either he has no other satellite than the four, or, if he has, they remain yet to be discovered."

Mr. Lassell's energy and zeal in the cause of science did not permit him to remain content with this magnificent telescope. His last work was a much larger telescope—four feet in aperture and thirty-seven feet focus, mounted equatoreally, which was erected at Malta in 1861. The work done with it in the next three years, during the last two of which he was assisted by Mr. Marth, is described, as well as the instrument itself, in vol. xxxvi. of the *Memoirs* of the Royal Astronomical Society. This work consists of numerous observations of nebulæ and satellites, and a catalogue of the places of 600 new nebulæ discovered at Malta. It is not possible to suppress a feeling of regret that this grand instrument no longer exists.

Mr. Lassell was very successful in the great brilliancy and permanence of polish in his mirrors. Within the last few years the writer has been shown specula by Mr. Lassell, which had been polished for more than twenty years, and which appeared as bright as if but just removed from the polishing machine. His earlier metal differed from that employed by Lord Rosse, in that it contained a small quantity of arsenic in addition to copper and tin. In the case of his four-foot mirrors the arsenic was omitted. He seems to have considered that the perfection of the metal depended upon the accurate relative proportion of the copper to the tin, and that in consequence of the uncertainty of the state of purity of the metals, this proportion would be obtained with the necessary accuracy only by a series of testings, while the metal was in the pot.

After his return from Malta, Mr. Lassell purchased a residence near Maidenhead, and erected there in an Observatory his

equatoreal telescope of two-foot aperture. Mr. Lassell's experience in re-polishing his four-foot mirror suggested to him some alterations in his polishing machine. After his return he was able to carry out experiments in connection with the suggested alterations in a workshop erected at Maidenhead, and succeeded in constructing an improved form of polishing machine, which is described in the *Philosophical Transactions* for 1874. The numerous papers by Mr. Lassell, to be found in the *Monthly Notices* and the *Memoirs* of the Royal Astronomical Society, bear abundant record to his industry and skill, and make us feel that in Mr. Lassell's death we have to deplore the loss of one who contributed largely to the advancement of the science of his age.

He was elected a Fellow of this Society on June 14, 1839, and received the Gold Medal in 1849 "for the construction of his Equatoreal Instrument and for the discoveries made with it;" the address, in presenting the medal, being delivered by the President, Sir J. F. W. Herschel. He was elected President of the Society in 1870, and, as usual, held the office for two years. He was for many years a member of the Council, and was a most regular attendant at its meetings up to his death.

He was elected a Fellow of the Royal Society in 1849, and received one of the Royal Medals in 1858. Among other honours bestowed upon him, may be mentioned the honorary degree of LL.D. conferred upon him by the University of Cambridge, and the honorary membership of the Royal Societies of Edinburgh and Upsala. W. H.

Rev. Robert Comyn Lumsden was born at Dalkeith, and studied at the University of Edinburgh. He was elected a Fellow of the Royal Geographical Society in 1860, and of this Society on May 10, 1861. He died of apoplexy, after a few hours' illness, on October 25, 1880, aged 56.

Benjamin Peirce was born at Salem, Massachusetts, on April 4, 1809. He was the son of Benjamin Peirce, the librarian of Harvard, who died in 1831, and is well known in connection with his posthumous History of that University. He graduated at Harvard in 1829, and was made Tutor in 1831 and Professor in 1833. In 1842 he was appointed Perkins Professor of Astronomy and Mathematics in the University, and occupied this Chair until his death. He also held the offices of Consulting Astronomer of the American Ephemeris and Nautical Almanac from 1853 to 1867, and Superintendent of the United States Coast Survey from 1867 to 1874. From 1874 to his death he was Consulting Geometer of the Coast Survey. He died at his house at Cambridge, Mass., on October 6, 1880, after an illness of about three months.

The proof sheets of Bowditch's translation of Laplace's *Mécanique Céleste* were read by Peirce. On p. 61 of the Memoir

of Bowditch, prefixed to the fourth volume (1839) of this elaborate work, it is stated that Peirce's "revision of the entire work, when in the process of publication, and vigilance in detecting typographical errors, Dr. Bowditch always valued as an additional means of ensuring its accuracy;" and on p. 140, "Whenever one hundred and twenty pages were printed, Dr. Bowditch had them bound in a pamphlet form, and sent them to Professor Peirce, who in this manner read the work for the first time. He returned the pages with the list of errata, which were then corrected with a pen or otherwise in every copy of the whole edition." The Royal Society's Catalogue of Scientific Papers contains the titles of fifty-nine papers by Peirce, the earliest of which appeared in the *Mathematical Miscellany* for 1838, and related to Fox Talbot's researches upon the Integral Calculus. Many of his papers were published in *Gould's Astronomical Journal* and in the *Proceedings of the American Academy of Boston*. In 1842 the first number appeared of the *Cambridge Miscellany of Mathematics, Physics and Astronomy*, edited by Benjamin Peirce and James Lovering; but only four quarterly numbers were issued. It chiefly contained solutions of problems. About 1845 he published text-books on Trigonometry, Geometry and the Differential Calculus; but his largest work of this kind was his *Analytic Mechanics*, which appeared in 1855, and was dedicated to the memory of "my master in science, Nathaniel Bowditch, the father of American Geometry." The *Mechanics* is a beautifully printed quarto volume, and covers a wide range of subjects; but the mathematics is somewhat embarrassed by the notations employed. The list of subscribers to this work shows the general estimation in which Peirce was held by his countrymen at that time. In 1853 Peirce's *Tables of the Moon* were published. These were described at length in the *Monthly Notices*, vol. xiv. pp. 26–32 (1853). Peirce was the author of memoirs upon the Saturnian system, the lunar theory, the discovery of Neptune, the rejection of doubtful observations in the Method of Least Squares, &c.; and he also published a treatise entitled *Curves, Functions and Forces*. One of the best known of his more recent writings is his *Linear Associative Algebra* (Washington, 1870), an account of which was given by Spottiswoode in vol. iv. p. 152 of the *Proceedings of the London Mathematical Society*. He was one of the founders of the American National Academy of Sciences. In 1853 he presided over the American Association for the Advancement of Science. He was elected an Associate of this Society on April 12, 1850, and a Foreign Member of the Royal Society in 1852. He received the degree of LL.D. in 1847 from the University of North Carolina, and in 1867 from Harvard University.

He leaves a widow, three sons and a daughter. Two of his sons, J. M. Peirce and C. S. Peirce, are well known in science. The former is Professor of Mathematics in Harvard University, and the latter is the author of investigations upon the applica-

tion of algebra to logic, and of photometric researches which were referred to in the Annual Report for 1879 (*Monthly Notices*, xxxix. p. 270).

CHRISTIAN FRIEDRICH AUGUST PETERS was born at Hamburg on September 7, 1806. His father was a merchant, who early remarked the ability of the boy for mathematical studies, and did his utmost to procure the necessary books for him. He attracted the attention of H. C. Schumacher, the director of the Observatory at Altona, who employed young Peters in computing ephemerides for his "Hülfstafeln," and for miscellaneous geodetical work. From the year 1826 Peters was employed under Schumacher's direction in the survey of the territory round Hamburg. Commencing with the same year, we find communications from him in the *Astronomische Nachrichten*, principally containing computations of orbits of comets and of occultations of stars.

Peters took his degree at Königsberg, under the illustrious Bessel; his dissertation bears the title, *Disquisitio de Motu Penduli in Aëre resistente.*

In the year 1834 he was appointed assistant at the Hamburg Observatory, the director being Ch. Rümker, who charged him with the observations with Repsold's Transit. Very few of the observations made by him with this excellent instrument are published.

A few years later W. Struve proposed him as one of the assistants to the director of the Pulkowa Observatory, then about to be finished. The Academy of St. Petersburg elected him to this office on December 15, 1838. He left his employment at Hamburg on October 1, 1839, and entered on October 11 upon his new duties at Pulkowa, where he immediately displayed great activity, jointly with his colleagues, Fuss, Sabler, and O. Struve, under the direction of W. Struve.

In recognition of his eminent services he was elected on February 5, 1842, an associate to the Academy of Sciences at St. Petersburg, and afterwards, on March 6, 1847, he was promoted to the rank of an extraordinary member.

His scientific papers written at that time and published in the volumes of the Academy, the *Recueil des Mémoires des Astronomes de Poulkova*, and the *Astronomische Nachrichten*, directed the attention of the scientific world to the eminent astronomer. He was elected an Associate of this Society on May 12, 1848, and received on February 13, 1852, the gold medal, the president, Professor J. C. Adams, delivering the address upon the occasion.

In the autumn of 1849 Peters returned to Germany, having left the Russian service. He was appointed Professor of Astronomy at the Königsberg University, and had besides secured the right to make use of the celebrated Heliometer of the Observatory. He did not, however, make many observations with this instrument. In his lectures at the University he had among his

pupils Radau and Marth. At Königsberg he wrote the memoir *Ueber die eigene Bewegung des Sirius*, and deduced therein the first orbit for the companion of this star, confirming entirely the views of Bessel, which had been opposed by W. Struve.

About this time Leverrier announced very sensible corrections to the positions of the fundamental stars as deduced from Bradley's observations by Bessel. The question being proposed for decision by the Dantzic Society, Peters obtained the prize by his memoir, *Die Abweichung des Greenwicher Passageninstruments in der Zeit von Sept.* 2, 1750, *bis Juli* 16, 1762.

In February 1854 Petersen, the director of the Observatory at Altona, who jointly with Professor Hansen of Gotha had after the decease of Schumacher edited the *Astronomische Nachrichten*, died. Peters was, after some months, appointed director of the Observatory and editor of the *Astronomische Nachrichten* by the Danish Government. He went to Altona in the autumn of 1854.

The first volume of the *Astronomische Nachrichten* edited by him is vol. xl., the last, vol. xcvi.

At Altona he commenced as early as 1855 to finish the reductions of Schumacher's geodetical works; for these computations he engaged in the autumn of 1855 Pape, afterwards his son-in-law. Instigated by this young and ardent astronomer, a new periodical was commenced by Peters under the title *Zeitschrift für populäre Mittheilungen*. After Pape's untimely death in 1862, it ceased to appear, three volumes having been issued.

The time of Peters was, in the last twenty years of his life, engrossed by many geodetical operations; the difference of longitude by means of the electric telegraph was determined under his direction between Altona and Schwerin, Altona and Kiel, Altona and Copenhagen, and Altona and Göttingen. He took also an active part in the yearly sessions of the International Committee for the European Triangulation, as the colleague of General Bayer.

After the termination of the Danish dominion in the German Elb-Duchies, the removal of the Observatory from Altona to Kiel, and its relation to the University, were duly considered. A new Observatory, near Kiel, was built after Peters' plan, and he commenced to reside there in 1872. Two years afterwards he was nominated Professor in the University. The new Observatory was partly equipped with the old instruments from Altona; but some new ones were procured. It is to be regretted that no description of them is published in books generally accessible to astronomers. It is only occasionally that observations made at this beautifully situated Observatory are now published.

Peters died on May 8, 1880, after a severe illness during the winter, leaving a widow, a son, assistant to his father in the Observatory, and a daughter, the widow of Dr. Pape. Several other children died long before him.

Proceedings of Observatories.

The following Reports of the proceedings of Observatories during the past year have been received by the Council from the Directors of the several Observatories:—

Royal Observatory, Greenwich.

The regular observations of the Sun, Moon, planets, and selected stars with the Transit Circle, and of the Moon with the Altazimuth, have been continued in the past year. A large number of phenomena of *Jupiter's* satellites, and 14 occultations of stars by the Moon have been observed, and the results have been communicated to the Society. In addition to observations of the beginning and ending of the Solar Eclipse of 1880, December 31, by several observers, a complete series of measures of differences of R.A. and N.P.D. of cusps and limbs of the Sun and Moon, was made during the eclipse with the S.E. Equatoreal. The measures were arranged in five sets, so as to give corrections to the tabular R.A. and N.P.D. of the Moon and to the adopted semi-diameters of Sun and Moon, and from 10 to 14 double observations of R.A. and N.P.D. were made in each set. These measures may be expected to give a very accurate determination of the Moon's place at an important part of her orbit.

Four determinations of the flexure of the Transit-Circle telescope, made during the past year under widely different conditions of temperature, confirm the change of sign remarked in 1879. The values found are $+0''\cdot19$, $+0''\cdot04$, $+0''\cdot04$, and $+0''\cdot20$. No correction for flexure has been applied since the beginning of 1879.

The horizontal circle of the Altazimuth has been re-graduated by Mr. Simms, the divisions having been nearly obliterated by repeated cleaning in the course of thirty years, and the division-errors have been determined for every 5°. To assist in the determination of zero of azimuth, a mark, consisting of a vertical slit illuminated by a reflector, has been fixed on the parapet of the Royal Naval College at a distance of about 1700 feet. This mark is now observed daily for azimuth-zero and error of collimation.

The spectroscopic observations have been made, as usual, with the Half-prism Spectroscope mounted on the S.E. Equatoreal. The chromosphere was examined on 39 days in 1880, prominences being seen on 38 of these days. On 9 days a more detailed examination of the spectrum of the chromosphere was made, the height, breadth, and relative brightness of all the chromospheric lines being recorded at every 15° round the Sun's limb.

Much attention has been given to the spectra of Sun-spots, and charts have been prepared showing, on 14 days of observation in 1880, the extent to which each of the lines in Ångström's maps between D and F is thickened over spots. The measures of displacement of the F or *b* lines in star spectra have been continued, and 81 stars have been observed during the year 1880, 28 of which had not been previously examined. Up to the end of 1880, the motions of 90 stars in the line of sight had been determined at Greenwich. All the spectroscopic observations have been completely reduced, and the copy for press has been prepared.

Photographs of the Sun have been taken on 160 days in 1880; on 23 there were no spots, and on 8 neither spots nor faculæ. As regards both spots and faculæ, a large increase is shown since 1879, accompanied by an increase in the mean distance from the Sun's equator. The photographs have been measured in duplicate up to the end of 1880, and the measures have been completely reduced and prepared for the printer.

A great advance has been made in the printing of the *Greenwich Observations* since the last Report. The volume for 1878 was printed and distributed last autumn, and the whole of the volume for 1879 has been sent for press. A large portion of the MS. for 1880 is in the printer's hands, and some proof-sheets have been received.

The delay in the appointment of an assistant to fill up the vacancy caused by Mr. Lynn's retirement in January 1881 has seriously crippled the Observatory; and though by an increase in the staff of computers the routine work has been kept up, there has been a great difficulty in supplying that critical examination on which the value of the results largely depends. Mr. Thomas Lewis has been recently appointed 2nd Class Assistant, the post having been vacant for exactly twelve months.

Cambridge Observatory.

Generally speaking the work has been of the same character as for several preceding years. The astronomical observations have been made mainly with the Transit Circle, and confined in great part to the zone lying between 25° and 30° of North Declination.

Of the Stars in this zone 2835 meridian observations have been made during the year, each of them determining both Right Ascension and Declination. For Clock Correction 620 observations of standard stars have been made, the West circle being read off at each observation, as the polar point for the zone stars has to be determined from these. In practice, for each zone, at least five standard stars, if possible, are observed.

For position of the instrument, *Polaris* has been observed 89 times above the Pole and 90 times below. The Circle is usually

read off several times at each observation, so that the 179 observations really include 454 circle readings, and, as we required a re-determination of the intervals of the Right Ascension wires, *Polaris* has been observed at all the wires when the weather has been favourable. The Nadir point is always observed at the close of each night's zone work, and at other times when it is judged necessary. It has been taken 174 times. There are 207 observations for collimation, and 173 for level. The observations for Nadir point and level are facilitated and rendered much more secure by an alteration in the mode of illuminating the wires through the Bohnenberger eye-piece. This was formerly done by means of a small lamp placed on a stand near to the eyepiece: it is now done by a much stronger light placed in the focus of a large lens at a distance of about 10 feet from the eye-piece. The improvement in the distinctness of the images is very striking, and the risk of heating the instrument much diminished.

A persistent effort was made to leave no zone star unobserved. This could have been effected probably before the close of the year had the unobserved stars been more uniformly distributed; as it is, there are still 723 stars in this class, and if the original determination to have at least three distinct observations of each star be carried out, 4742 observations will still be required in order to complete the zones.

The Reductions progress as rapidly as our resources will allow. The means are taken, and the correction for runs of microscopes applied to the end of 1880, and corrections for micrometer readings are calculated up to November 20. The Nadir points and corrections for level and collimation are calculated up to May 20. A large number of observations of *Polaris* are tabulated for the determination of intervals of R.A. wires, but, from the pressure of other work the final results have not yet been obtained. The true Right Ascensions and true North Polar Distances of the zone stars are calculated up to the end of 1878, and the calculations requisite to form the tables for reducing apparent place to mean are performed up to near the end of 1876.

The Northumberland equatoreal has been used in the occasional examination of planets and double stars, in the search for intra-mercurial planets and Faye's Comet, and in the observation of the Solar Eclipse of December 31.

The meteorological observations are regularly taken, and communicated every morning, by telegraph, to the Meteorological Office in London.

Dunsink Observatory.

In the last Annual Report it was mentioned that a series of observations of the Annual Parallax of 61 (B) *Cygni* had been completed. These have now been reduced and give for the

parallax $+0''\cdot4676 \pm 0''\cdot0321$. The comparison star is B.D. $+38°$, 4351.

A second series of observations of true position and distance of B.D. $+50°$, 1724 from Groombridge 1618, which was commenced on August 14, 1879, was concluded on July 29, 1880. Observations were made on 40 nights. The parallax concluded from the observations of distance in this series is $+0''\cdot3196 \pm \cdot052$. The former series has also been reduced in the past year, and the resulting parallax from distance is $+0''\cdot3472 \pm \cdot05$. The observations of position-angle must receive further consideration.

The parallax series for P. III. 242, commenced on January 11, 1879, was concluded on January 9, 1880. Observations were obtained on 29 nights. The result of the series is $\pi = -0''\cdot045 \pm 0\cdot070$. (See *Monthly Notices*, November 1880.)

A parallax series for the red star 249 (*a*) of Schjellerup's list (B.D. $+34°$, 4500) was commenced on November 25, 1876, and concluded on October 20, 1880. Observations of the distance and position of two comparison stars were obtained on 20 nights. The reductions have not yet been completed.

A parallax series for μ *Cephei* was commenced on August 19, 1880, with two comparison stars. This series is at present in progress. A series for Σ 2486 was commenced on October 3, 1880.

Observations of the following objects have been completed within the past year by the method given in "Dunsink Observations," Part iii. The results show that probably all these objects are more distant than 61 *Cygni*:

Schj. (Red) 83; *Schj.* (Red) 87; B.D. $+19°$, 2526; B.D. $+35°$, 2462; *S. Delphini*; B.D. $+64°$, 1713; B.D. $+65°$, 1814; *Schj.* (Red) 269; σ *Cassiop.*; *Arg.* 49; *Arg.* 61; *Schj.* (Red) 1; *Schj.* (Red) 6; B.D. $+52°$, 262; *Schj.* (Red) 7 *a*; *Schj.* (Red) 20 (*a*); B.D. $+47°$, 779; *Oeltz. Arg.* 4090; *Schj.* (Red) 31; *Schj.* (Red) 41; ζ *Aurigæ*; *Schj.* (Red) 56 (*a*); *Schj.* (Red) 191 (*a*); B.D. $+38°$, 3362; B.D. $+24°$, 3914; B.D. $+22°$, 4203; B.D. $+36°$, 4028; B.D. $+40°$, 4141; 47 *Cygni*; B.D. $+32°$, 3980; *Schj.* (Red) 264 (*a*); *Schj.* (Red) 267; π *Andromedæ*; 56 *Andromedæ*; Σ 3042; ψ *Andromedæ*; 14 *Aurigæ*; 16 *Aurigæ*; B.D. $+31°$, 1111; Σ 872; Σ 644; B.D. $+31°$, 2962; B.D. $+52°$, 2058; B.D. $+48°$, 3133; B.D. $+40°$, 4373; B.D. $+43°$, 4375; μ^1 *Boötis*; 44 *Boötis*; P *II.* 72; ψ *Cassiop.*; P *XXIII.*, 101, 100; Σ 163; *Schj.* (Red) 241; B.D. $+43°$, 3422; Σ 477; Σ 865; Σ 866; Σ 876; Σ 878; Σ 884; Σ 896; Σ 905; Σ 904; Σ 916; Σ 918; Σ 928; Σ 922; Σ 3115; ϕ *Cassiop.*; ζ *Coronæ*; η *Coronæ*; o^2 *Cygni*; χ^1 *Cygni*; ω^3 *Cygni*; ψ *Cygni*; 52 *Cygni*; P *XIX.*, 276; P *XIX.*, 149; Σ 2705; Σ 2666; Σ 2607; Σ 2708; 32 *Cygni*; ν^1 *Draconis*; 39 *Draconis*; Σ 2420; Σ 2078; Σ 2348; Σ 2403; 46 *Draconis*; Σ 2082; Σ 2922; Σ 2894; Σ 946; Σ 958; Σ 1338; β *Lyræ*; ζ *Lyræ*; ϵ^1 *Lyræ*; η *Lyræ*; 17 *Lyræ*; Σ 2472, 2473; δ^1 and δ^2 *Lyræ*; θ *Lyræ*; Σ 2372; *B. A. C.* 6468; Σ 331; 12 *Persei*; 57 *Persei*; Σ 533; Σ 1561;

Σ 1415; B.D. +54°, 1556; Σ 1559; Σ 791; Σ 794; Σ 802; Σ 1024; Σ 1025; Σ 1033; Σ 1039; Σ 1050; Σ 1054; Σ 1070; Σ 1075; Σ 2291; Σ 2305; Σ 2340; Σ 2349; Σ 2378; Σ 2376; Σ 2393; Σ 3130; Σ 2421; Σ 2431; Σ 2441; Σ 1462.

The meridian observations of red stars were finished last summer, and are now in preparation for printing. It is contemplated to devote the meridian circle, in connection with the new chronograph, to a re-examination of Argelander's southern zones between −15° and −20°.

Royal Observatory, Edinburgh.

During the past year the daily electric time-signals of ball and gun, together with supervisions of controlled clocks over Edinburgh city, have been carried out, with the usual attention to everything connected with their regularity and astronomical truth. The bi-diurnal meteorological observations at 55 stations of the Scottish Meteorological Society have also been computed and discussed for the Registrar-General of Births, Deaths, &c. in Scotland, and have been printed by that officer in his monthly and quarterly reports.

A meeting of the Board of Visitors was held in April, at which steps were taken to endeavour to induce the Government to have the requisite instrumental renovations, &c., carried out as soon as possible.

The first assistant astronomer, Mr. Alexander Wallace, M.A., having reached the age of 77, after a hard and continuous service of 46 years, has resigned.

Glasgow Observatory.

The astronomical operations at the Glasgow Observatory during the past year have been devoted almost wholly to the laborious work of preparing for the press the Star Catalogue alluded to in the last Annual Report of the Council. The Catalogue is now being printed, and Professor Grant expects that it will be ready for publication early in the spring of next year.

Kew Observatory.

The preliminary reductions of the measurements of the Kew solar negatives having been completed in January last, a re-examination of the pictures was made with the object of classifying the spots according to a scale of figure and magni-

tude. This being now terminated, the reduction to heliocentric elements of the pictures from January 1864 to April 1872 is rapidly progressing.

These operations have all been conducted under the direction and at the expense of Mr. De La Rue.

The eye observations of the Sun, after the method of Hofrath Schwabe, have been made on 246 days, in order to maintain for the present the continuity of the Kew records of Sun-spots. The Sun's surface was observed to be free from spots on 27 of those days.

A catalogue of the whole of the solar photographs taken at Kew during a decade, 1862 to 1872, has been prepared and forwarded to the Solar Committee of the Science and Art Department.

At the request of the Council of the Royal Astronomical Society, the valuable collection of MSS. containing the memorable series of Sun-spot observations made by Hofrath Schwabe, of Dessau, during the years 1825 to 1867, which had been deposited in the Library of the Observatory—the first volumes since 1865—was transferred to the Society's Library at Burlington House, London. In order, however, to render the collection of Sun-spot observations at Kew as complete as possible, and to prevent the total loss of the observations in case of fire, the Committee ordered a complete copy of the solar drawings to be made.

This was accordingly done, and accurate tracings made of every one of Schwabe's drawings. These were pasted into blank books, and any important notes were transcribed at the same time.

The Observatory, therefore, now possesses a complete record of the condition of the Sun's surface, extending from November 1825 to the present date.

The Magnetical and Meteorological Observations, which principally form the work of the Observatory, have been continued as in former years, and experimental work and the verification of instruments have also largely occupied the attention of the staff.

Liverpool Observatory, Bidston, Birkenhead.

In this Observatory the Mersey Docks and Harbour Board have provided very perfect arrangements for the testing of nautical instruments. The owner or commander of any ship for which dock rates are paid in the Port of Liverpool may have the ship's chronometers tested gratuitously in three definite temperatures by sending them to the Observatory. This privilege is also extended to chronometer-makers, who are allowed to send a limited number of nautical timekeepers periodically to be tested.

A considerable proportion of the chronometers tested during the past three years have the words "Auxiliary Compensation" engraved on their dials; but the average thermal error in these instruments has been found to be quite as large as that found in timekeepers in which no deviation from the ordinary construction is said to have been made.

With the means provided at this Observatory by the Mersey Docks and Harbour Board the thermal error can be very accurately obtained, and the application of corrections to the rates, due to change of temperature, is now rendered so easy that the mariner finds no more difficulty in using a variable rate dependent on the temperature than in using one uniform rate for all climates. The correction of the rates for thermal error has led to the discovery of a small, but very systematic, difference between the sea and shore rates of chronometers. With very few exceptions, chronometers used in steamships have been found to go slower at sea than on shore, the amount in different instruments ranging from a small fraction of a second to upwards of one second a day. This change of rate is probably caused by the motion of the ship. It is small in comparison with the thermal error, and is only shown in cases where the corrections for change of temperature have been carefully applied at sea during the voyage.

The regularity of the performance of chronometers is not such as to admit of the thermal error being found in a few days. In some timekeepers five weeks is found to be sufficient for a fairly good result to be obtained, but in others a much longer period is required.

Steamships, as a rule, are only a short time in port, and although it is very desirable to watch the performance of the chronometers during this period, so as to ascertain that no great change has taken place, the rates so obtained are found to be only rough approximations to the rates made by the timekeepers during the subsequent voyage. When a careful record is kept of the temperatures to which chronometers are exposed at sea, and the corrections due to change of temperature are applied, the differences between calculation and observation on the preceding voyage are found to be the best corrections to apply to the old rates in order to find the most probable rates for the next voyage. The average error for the mean of three fairly good chronometers treated in this way is found to be between three and four miles of longitude on the equator for a voyage of one hundred days.

Radcliffe Observatory, Oxford.

The Transit Circle has been cleaned, readjusted, and brought into regular use during the past year. 1270 observations of

Right Ascensions and 2059 Circle observations have been made and reduced. The Moon has been observed 50 times on the meridian.

132 measures of the position-angles and distances of double stars, and some measures of the diameters of *Mars* have been made with the Heliometer. These measures have been made with the reversions necessary to secure accuracy with this instrument.

The Solar Eclipse of 1880, December 31, was well observed here, and some measures of the chords made with the Heliometer. Ceraski's variable was observed on 13 nights, and continuously watched for 6 nights.

The photographic meteorological instruments have been renewed, and are now working in a perfectly satisfactory manner. The volume of Astronomical Results for 1876 has been completed, published and distributed, and a volume containing the Meteorological Observations 1876–1879 has also been published.

Oxford University Observatory.

The astronomical work undertaken since the last Annual Report in the *Monthly Notices* is as follows:—

1. Observations of the parabolic comets, Schäberle, Hartwig, Swift and Pechüle; of the periodic comet of Faye fourteen nights' observations have been secured.

2. Observations of 280 stars in the Group 39 Messier have now been completed. It was found necessary to reobserve the difference of Right Ascension with an altered inclination of the micrometer bar to the parallel, in order to secure as much precision herein as was obtained for the difference of N.P.D. with the original and less inclined position. The reductions are in a forward state. It may possess some interest to astronomers to be informed that a very successful application of the driving clock of the Grubb Equatoreal to a modified form of the Morse Telegraph has been made, whereby a very serviceable and accurate chronograph has been obtained, which has been successfully used in the observations of the forementioned group of stars. A chronograph of some form seems indispensable for such measures.

3. Observations of the Binary Stars ξ *Ursæ Majoris* and 70 *Ophiuchi* have been made, with the view of ascertaining how far they accord with the places of the components as computed from the elements of the orbits obtained by the somewhat novel process adopted in this Observatory. The results in the case of ξ *Ursæ Majoris* are of a highly satisfactory character. In passing over an arc of 50° since the orbit was computed there is, after the lapse of 6 years, a discrepancy of less than a degree in position-angle, and of less than a tenth of a second of arc in

distance. This is, it is believed, a closer degree of accord than applies to any other set of elements hitherto obtained. In the case of 70 *Ophiuchi* the accordance of the observed and computed position-angle is as satisfactory as in the foregoing case. Particulars will be communicated to the Society at the March meeting.

4. The computations for the Lunar Physical Libration are now completed, and the main result has been communicated to the Society. The general account of the whole process is in course of preparation for the press.

5. In the October term it was thought advisable to invite members of the University to visit the Observatory during the somewhat remarkable apparition of the planets *Jupiter* and *Saturn*. It was gratifying to find that very nearly 300 students and senior members of the University availed themselves of the opportunity. A few occasional lectures on the phenomena presented were given on successive evenings. In conjunction with Mr. Grubb, a very inexpensive form of small Equatoreal Telescope, of either $2\frac{1}{2}$ or 3 inches' aperture, has been arranged for incipient astronomers. This instrument is found, on trial, to be admirable in its optical and mechanical performance.

Stonyhurst Observatory.

The astronomical work at this Observatory has been considerably increased during the past twelve months, but without interfering in the least with the meteorological and magnetic departments. The large automatic spectroscope, which came into daily use in November 1879 has been employed for the measurement of the chromosphere on every favourable occasion; and a special assistant has been appointed to the duty of studying the chromosphere, and of making a daily sketch of all Sun-spot phenomena. The whole chromosphere has been measured on 45 occasions, and parts of it on 19 other days. The highest prominences were seen in November, when they attained a maximum height of 2′28″. The dispersive power of the spectroscope has been increased lately by the addition of a Christie half-prism, and now the greatest dispersion is equal to that of 36 prisms of 60°.

The new sunshine recorder, which was erected in March 1880, has served, in connection with the spectroscope, to show the large number of days on which the sun may be shining brightly, and yet the state of the atmosphere prevent any reliable measures of the chromosphere.

The observation of *Jupiter's* satellites and of occultations of stars by the Moon has been continued as usual.

A long search for the ultra-Neptunian planet, indicated by the researches of Professor G. Forbes and Mr. D. P. Todd, has

not as yet resulted in the detection of any body that has done more than raise a passing suspicion of its planetary nature. The observations are being continued on every clear moonless night.

On December 31, preparations were made for observing chromospheric contacts, and also for taking a series of photographs of the Sun during the progress of the partial eclipse, but a cloudless morning was followed by a sunless afternoon, and a momentary break in the clouds afforded the only glimpse of the Sun in the course of the afternoon. This, however, sufficed to show that the first contact must have been at least some minutes later than the computed time. The weather was equally unfortunate during the week of the November meteors.

During the past year a large automatic spectroscope has been sent to the new Observatory of Kalocsa, and other instruments to the observatories of Manila and Trikawei.

Temple Observatory, Rugby.

The work of the past year has been chiefly the measurement of positions and distances of double and multiple stars, 421 complete sets of measures having been made by Mr. Seabroke, Mr. Hodges, and Mr. Percy Smith with the Alvan Clark Refractor.

The results of double-star observations of the last three years have been prepared for publication, and will appear in the next volume of the *Memoirs* of the Society.

A number of measures of position of a few double stars, whose angles are fixed or nearly so, have been made with the observer's head in various positions, and while making various angles with the horizon for the detection and estimation of errors in measuring the position of double stars. Speaking generally, from what has been already done, there is a tendency to place the position wires out of parallelism with the line joining the stars in the direction of the horizon, irrespective of the position of the observer's eyes.

Some few measures of the velocity of motion of stars in the line of sight have been made with the spectroscope, having a power of 6 prisms of 60° on the reflector.

The Observatory has been open for the usual hour every fine evening for members of the School, who are taught the uses of the equatoreal and transit.

Mr. Barclay's Observatory, Leyton, Essex.

During the past year the work has been carried on in a systematic manner, the main feature being observations of binary stars. Numerous comet observations have been made, as well

as observations of *Jupiter's* and *Saturn's* satellites. A new wire micrometer, by Cooke, it is hoped, will soon be added to the list of instruments.

Mr. Campbell's Observatory, Arkley, Barnet.

During the past year considerable progress has been made in observing the transits of the lunar Crater Murchison A, with the view of determining the value of the parallactic inequality in the motion of the Moon, so as to obtain a better determination of the Solar parallax.

The extremely unfavourable weather has materially retarded the work; but during the year 53 observations were obtained, and are now fully reduced.

During the latter part of the year, whenever possible, observations were made of both the limb and the crater, so as to enable comparisons to be made between the apparent errors of the Moon as determined by each class of observation.

Through the courtesy of Professor Winnecke, who has had corresponding observations made at Strassburg, it has been possible to compare the Arkley observations with simultaneous observations made at Strassburg. At both Observatories the conclusion has been reached that the observations of a crater on the Moon can be made with far greater accuracy than those of the limb—in fact, with an accuracy surpassing that of a star.

During the year a new collimating telescope has been designed and mounted, with the view of more certainly determining the error in both collimation and azimuth of the Transit instrument. It is proposed to carefully test this during the coming year, to see if the practical advantages are as great as were anticipated.

A series of observations has also been commenced with the Alvan Clark Equatoreal, with the object of determining the inequalities on the limb, and their influence on the apparent error of the Moon as determined by observations of the transit of the lunar limb. The same series of observations will also serve to determine the variation in the irradiation of the Moon's limb as seen under different conditions.

Mr. Common's Observatory.

During the past year the weather has not been at all favourable for astronomical observations. Faye's Comet was found on August 8, 1880. Swift's Comet has been observed to January 5, 1881, with the 36-inch reflector. Photographs of *Jupiter* have

been taken showing the shadow of the third satellite on the disc. The 18-inch and 36-inch telescopes have been taken down, and the 36-inch erected on a new site some four hundred yards further north—the position of the Observatory from the Ordnance Map being now, lat. N. 51° 31′ 7″, long. W. 1′ 13″.

Colonel Cooper's Observatory, Markree.

During this year observations have been made of a number of the most interesting binary stars from a working catalogue, the contents of which it is expected will be finished off in the course of the coming year. The magnitude and colour of red stars in Birmingham's Catalogue have been also noted. At the end of the year some observations of comets were made with the old square bar micrometer. The dimensions of some major planets have been measured, and the phenomena of their satellites, as well as the spots on their surface, have been observed. A sunshine recorder was at the commencement of the year added to the meteorological instruments.

The Earl of Crawford's Observatory, Dun Echt.

The Transit Circle has been used during the past year for finding the places of a few comparison stars, as well as for determining the time. On April 9, the telescope, which had originally been focussed on the pole-star, was re-adjusted with a Bohnenberger eye-piece on the reflected images of the wires as seen in quicksilver. The slight change thus made has greatly improved the visibility of the reflected images in making nadir observations without injuring the definition on stars. A time-gun is fired every Saturday at Greenwich mean-noon.

The 15·06-inch refractor is in thorough working order, the electrical control acting perfectly under great changes of load in all temperatures. The great gale of 1879, December 28, partly displaced the dome; but the addition of an extra pair of guide-wheels has, it is believed, made the structure capable of withstanding a much more violent storm. The following comets have been observed for place:—1880 *b* (Schäberle) 10 times; Faye's Comet, once; *e* (Hartwig) 5 times; and Swift's second comet, *f*, 14 times. This last comet was also independently discovered at Dun Echt by Mr. J. G. Lohse on November 7. The spectrum of Hartwig's Comet was observed on 3 nights; that of Swift's Comet, which only showed a faint trace of one band, and that of Pechüle's comet, *f*, were each observed on one night. An orbit of the great Southern Comet, *a*, was computed from three

rough observations received from Mr. Gill, and its probable identity with the great comet of 1843 pointed out. On this basis a search was kindly made by Mr. Pratt at Brighton, and also at Dun Echt. We now know that, although near the place indicated, the comet was too faint to be seen. Orbits and ephemerides of comets *b* and *c* were computed and, as well as those of *a*, communicated to the *Astronomische Nachrichten*. The first news of the comet seen by Mr. L. Swift on August 11, reached Dun Echt twenty days afterwards in No. 2331 of the *Astr. Nachr.*; a long but unsuccessful search was made. Another comet, announced by Mr. Cooper of Sheffield, was also sought for in vain.

A number of sweeps have been made with a prism between the eye-piece and the object-glass on the plan devised by Professor E. Pickering. In this way Dr. Copeland discovered a small, bi-nuclear, planetary nebula on November 18; its spectrum contains a fourth line distinct from the fourth line of the spectrum of the great nebula in *Orion*.

Some spectra of variable stars have been examined and a few double stars and other objects measured. The chief results have been published in the *Monthly Notices*, *Astronomische Nachrichten*, or *Urania*.

Thirteen Dun Echt circulars have been distributed during the year. Of these 1 related to a supposed trans-Neptunian planet, 2 to variable stars, 1 to the new nebula, and the rest to comets.

In the laboratory Mr. Lohse has determined the optical constants and specific gravities of 10 sample prisms. The whole of the laboratory and library are now warmed by hot-water pipes. The book-shelving has been greatly extended.

In September, M. Perrotin, of Nice, visited the Observatory, devoting two days to examining the instruments, &c.

Mr. Edward Crossley's Observatory, Bermerside, Halifax.

No change in the class of observations made here has taken place since the last Report. As before, the measurement of double stars, the observation of the phenomena of the satellites of *Jupiter* and *Saturn*, sketches of *Jupiter*, transits of the "red spot" on *Jupiter*, and the daily reading of the usual set of meteorological instruments, constitute the work of this observatory.

The instrument used is the same as in previous years—viz. the $9\frac{1}{2}$-inch refractor by Cooke. About a year ago the edge of the declination circle, which is $\frac{3}{4}$-inch broad, was divided boldly into degrees, with white paint on a black ground so as to be easily seen from the eye-end of the telescope. A vernier was found unnecessary, an index point being quite sufficient. Parts

of a degree are easily set by estimation, so as always to bring the object into the centre of the finder. This has been found so convenient in practice that we wish to draw the attention of observers who still go on setting for an object by means of the finely divided declination circle to the great saving of time and trouble secured by the above arrangement. In our case an ordinary lamp held in the hand enables us to set the instrument quickly in declination without the aid of reading microscope or telescope of any kind.

The observations of the satellites of *Jupiter* and *Saturn* for the year ending December 31, 1880, are now ready for presentation to the Society, and the double-star measures made here since 1876 will shortly be ready.

Mr. Huggins's Observatory, Upper Tulse Hill.

During the past year more perfect photographs of the spectra of *Rigel*, *Betelgeux*, *Aldebaran* and *Capella* have been obtained.

In the arrangement described in the last Report *Rigel* will have its place between *α Cygni* and *α Aquilæ*.

The photographic spectrum of *Capella* resembles the solar spectrum very closely, and appears to be a star in the same physical stage as our Sun.

The spectra of *Betelgeux* and *Aldebaran* are very difficult to photograph. In the case of *Betelgeux* an exposure forty times greater than would have been necessary for a strong spectrum of *Sirius* gave but a faint spectrum of limited extent. The photograph is strongest about G, and ends abruptly a little beyond H.

The spectrum of *Aldebaran*, with an exposure fifty times greater than *Sirius* would have required, extends from about F to H, with a faint trace of the more refrangible region. About fifty lines may be measured in this spectrum.

There seems good reason to believe that these photographic spectra indicate some of the life-changes through which the stars pass, and that the stars observed may be arranged in the following order:—

Sirius . . . Vega.	α Cygni.
η Ursæ Majoris.	. . .
α Virginis.	Capella. The Sun.
α Aquilæ.	Arcturus.
Rigel.	Aldebaran.
	Betelgeux ?

Attempts have been made to photograph the spectrum of the Nebula in *Orion* and of the ring Nebula in *Lyra*, but hitherto

without success. Further attempts, with an altered form of apparatus, are in progress.

In the laboratory a remarkable group of lines in the ultra-violet (W. L. 3276 to W. L. 3062) was photographed in the flames of hydrogen and of several hydro-carbons. This group represents the spectrum of water in the form of luminous vapour, and explains the remarkable power of exciting fluorescence which was observed by Professor Stokes in these flames in the year 1852.

The Earl of Rosse's Observatory, Birr Castle.

During the year 1880 some observations of nebulæ were made by Mr. Keating up to April 1, when his engagement terminated. Work was not resumed until after November 1, when Dr. Boeddicker, late of the Hamburg Seewarte, undertook the duties. He has since made some sketches of the planet *Jupiter*, and some preliminary determinations of lunar heat have been made with the view of ascertaining what probability there is of the subject being further advanced, now that the 3-foot telescope is provided with an equatoreal mounting, and by taking further precautions against errors. Observations have been recorded on 35 nights during the year.

The meteorological observations at 9 a.m. and 9 p.m. have been continued regularly, also at 8 a.m. and 6 p.m. Greenwich time for telegraphic reports.

Since the last Annual Report, Part III. of the "Observations of Nebulæ and Clusters," &c., has been published, and a good many copies distributed. Should any person who has received Parts I. and II. (sewed together), but has not received Part III., or *vice versâ*, notify the fact, the missing part will be sent. Part III. completes the work.

Colonel Tomline's Observatory, Orwell Park.

The work of the past year at this Observatory has been continued upon much the same lines as in former years. All the comets that have been visible have been more or less observed, and especially good series of Schäberle's and Hartwig's Comets have been secured. The former was kept in view until September 9, and the latter until November 25. Pechüle's Comet is still under observation at the end of the year.

The completed observations of Brorsen's Comet in 1879 were supplied to the *Astronomische Nachrichten* early in the year,

but there yet remain considerable arrears of cometary work which have not been fully reduced. Efforts will be made at once to prepare these for the press, along with those of 1880, with the view to their speedy publication.

The eclipse of the Sun on December 31 was well seen, but unfortunately the state of the atmosphere did not admit of very accurate observation.

A number of occultations of stars by the Moon have been observed, and it is proposed in future to watch for the unpredicted occultations of small stars at the dark limb of the Moon during the early days of each lunation, in the hope of detecting and elucidating the phenomenon of projection on the limb. Some trials already made lead the observer to believe that a considerable number of occultations may in this way be observed in a comparatively short space of time at various points on the limb.

Notes on some Points connected with the Progress of Astronomy during the Past Year.

Discovery of Minor Planets.

During the past year the following additional minor planets have been discovered, thus increasing the number of the group to 219:—

Number.	Name of Planet.	Date of Discovery. 1880.	Name of Discoverer.	Mean Distance.
(213)	Lilæa	February 16	Peters	2·746
(214)		March 1	Palisa	2·611
(215)	Œnone	April 7	Knorre	2·768
(216)		April 10	Palisa	2·794
(217)		August 30	Coggia	3·051
(218)		September 4	Palisa	2·702
(219)		September 30	Palisa	2·382

Several of the minor planets discovered in 1879, as well as in the past year, have not yet been named.

M. Niesten has given in the *Annuaire de l'Observatoire Royal de Bruxelles* an interesting *résumé* of the relative elements of the orbits of the first 216 minor planets, classifying them according to their sidereal periods, longitudes of the perihelion and node, excentricities, inclinations, and mean distances from the Sun, with general remarks on their magnitudes, and on the probable origin of the group. From one of these tabular statements the following numbers are extracted, from which a clear notion is obtained of the relative mean distances of the minor planets, and of the breadth of the zone in the solar system occupied by this remarkable group of planets. Taking the mean distance of the Earth as unity, the mean distances of *Medusa*, *Flora*, and *Ariadne*, the three nearest minor planets, lie between 2·00 and 2·25, while forty-five lie between 2·25 and 2·50, seventy-one between 2·50 and 2·75, thirty-nine between 2·75 and 3·00, thirty-eight between 3·00 and 3·25, seven between 3·25 and 3·50, and two between 3·50 and 4·00. It will thus be seen that the great mass of minor planets are included within a zone extending from 2·25 to 3·25, and that in this zone the mean distances of the greater number lie between 2·55 and 2·80, showing, as might be expected, that the inner portion of the ring of known asteroids is comparatively richer than the outer portion. E. D.

The Comets of 1880.

Six comets, including the periodical comet of Faye, have been satisfactorily observed during the past year. A few brief details of each are given below, in the order of their discovery or first observation.

1. *The Great Southern Comet*, 1880 (*a*).—This remarkable comet was a conspicuous object in the sky of the southern hemisphere during the first half of February. The first announcement in Europe of its discovery was a telegram dated February 5, sent by Dr. Gould, of Cordoba, to the late Professor C. A. F. Peters, stating that a great comet was passing the Sun northwards. This was followed by a second telegram, dated February 6, to the effect that the comet was then moving southwards. This uncertainty in the direction of the comet's motion was not explained till the independent announcement of its discovery was received by mail from the Cape. The tail appears to have been noticed on the evening of February 1, by several persons in Australia, South America, and at the Cape; but it was not recognised as a comet till the following evening, when sketches and observations were made at Cordoba by Dr. Gould; at Sea Point, west of the Table Mountain, Cape of Good Hope, by Mr. Gill; at Melbourne, by Mr. Ellery; at Adelaide, by Mr. Todd; and at Castlemaine, Victoria, by Dr. Bone. The tail was also noticed by other observers in Australia, South America, and at the Cape.

According to Dr. Gould's observations, the tail of the comet appeared as a luminous band, extending from the horizon in the S.S.W. about forty degrees towards γ *Toucanæ*, passing near δ and β *Gruis*, and about $1\frac{1}{2}$ to $2\frac{1}{2}$ degrees in breadth. On February 4 it could be traced as far as α *Eridani*. On this day the nucleus was seen by Dr. Gould at Cordoba, and by Dr. Bone at Castlemaine, but it was excessively faint. At Melbourne it was not observed before February 9, and at Sydney before February 13. At the Cape, owing to the amount of haze near the horizon, and the small optical power of the instrument employed, the nucleus was not seen at Sea Point where the tail was first noticed; and even at the Royal Observatory, Cape Town, Mr. Gill failed to see anything like a nucleus till February 9. From this day to February 15 observations were secured daily by Mr. Gill and Mr. Finlay with the $8\frac{1}{2}$-feet equatoreal. The nucleus continued to be observed by Mr. Ellery till February 17, and by Dr. Gould till February 19. Various sets of elements of the orbit of the comet have been calculated from the observed positions of the nucleus by several persons, including Dr. Copeland, Mr. Finlay, Dr. Gould, Mr. Hind, Mr. Meyer, and Mr. Tebbutt. The elements bear a close resemblance to those of the Great Comet of 1843, also celebrated for its length of tail, which

alone was visible in England, where the nucleus was below the horizon during the apparition. There seems a possibility that the two comets may be identical, but whether they are so or not must remain for the present an open question. For more details respecting the Great Southern Comet the reader is referred to the observers' statements in the *Monthly Notices*, Vol. xl. Nos. 5, 6, and 9, Vol. xli. No. 2, and in the *Astronomische Nachrichten*, Nos. 2307 and 2308. Professor Kirkwood has made some interesting speculations on the supposed identity of this comet with the comet of 1843 in the *Observatory*, Vol. iii. p. 590.

2. *Schäberle*, 1880 (*b*).—This comet was discovered by Mr. J. M. Schäberle, assistant at the Observatory at Ann Arbor, U.S., shortly before midnight on April 6. It was very faint, its nucleus being about the tenth magnitude, accompanied by a tail about 3′ in length. It was well observed, both in the United States and Europe, and several sets of elements have been calculated. Those by Mr. Hind, derived from an observation made at Strassburg by Professor Winnecke, on April 11, and two observations by Lieut.-Col. Tupman at Blackheath, on April 27 and May 8, will be found in the May number of the *Monthly Notices*. Elements by Professor Safford and M. Bigourdan are inserted in the June number.

3. *Faye*, 1880 (*c*).—This excessively faint but interesting periodical comet was observed by Mr. Common, at Ealing, on several occasions, with his 3-feet telescope. He remarks that "when first seen, on August 2, 10^h, it was faint, round, and a little brighter towards the middle." This comet has also been seen by several observers, including M. Tempel, at Florence; M. Dunér, at Lund; Dr. Pechüle, at Copenhagen; and M. Bigourdan, at Paris. It was detected by the aid of Dr. Axel Möller's excellent ephemeris, the observed errors of which have been found to be no greater than about $2^s{\cdot}5$ in Right Ascension, and 2″ in Declination. The President will explain in his address the principal points of interest in Dr. Möller's important researches on the orbit of this comet.

4. *Hartwig*, 1880 (*d*).—This comet was discovered by Dr. Hartwig, at Strassburg, on September 29, with a brilliant nucleus and a tail two degrees in length. It was discovered independently at Ann Arbor, by Professor Harrington, on September 30, and by Mr. Baxendell, at Southport, on October 3. This comet, which passed its perihelion on September 6, was favourably observed at most of the principal observatories. At Brussels it appeared round, with a diameter of about three or four minutes, and a nucleus equal in brightness to a star of the fifth magnitude. The comet was visible in telescopes with small aperture, and it was occasionally seen by the naked eye. Its spectrum was examined by several observers. At the Royal Observatory, Greenwich, it was observed on three separate days, when it was found to consist of the three usual cometary bands, the nucleus giving a continuous spectrum. Some valuable spectroscopic ob-

servations of this comet, made by Professor C. A. Young, may be found in the December number of the *Observatory*, vol. iii. p. 645.

Professor Winnecke has suggested the possibility of this comet being identical with the comets of 1382, 1444, and 1569, and perhaps with that of 1506, from which he assumes a probable period of 62⅓ years.

5. *Swift*, 1880 (*e*).—This comet was discovered by Professor Lewis Swift, of Rochester, N.Y., on October 10, but owing to unfavourable weather and want of information, it was not observed in Europe till October 25, when M. Block, at Odessa, picked it up. Dr. Lohse detected it independently at Dunecht, on November 7, believing that the object was a new comet. It had, however, been observed in America on October 21, 25, and 28, and Mr. S. C. Chandler, jun., was thus enabled to calculate the elements of its orbit, which agree sensibly with those of Comet III. 1869 (Tempel). There seems to be no doubt that the two comets are identical. Mr. Chandler has also discussed all the observations of Comet III. 1869, 39 in number; and from three normal places he has deduced elements on three different hypotheses of a parabola, an ellipse of 4,006 days (or about 11 years), and an ellipse of 2,003 days (or about 5½ years). From the smallness of the residuals in the third hypothesis, Mr. Chandler concludes that the true period will prove to be about 5½ years. MM. Schulhof and Bossert have arrived independently at a similar conclusion, from a discussion of the observations of the comet at the present appearance, from October 25 to December 5. The agreement of the different sets of elements is perfect, and leaves little doubt of the accuracy of the conclusion, which is further confirmed by elements calculated by Prof. E. Frisby from three observations made with the Transit Circle at Washington by Prof. Eastman on October 25, November 7 and 20, in which no assumptions about any periodic time have been made. The period found from these observations is about 2178 days, or a little less than six years (*Astron. Nachr.* No. 2359). An excellent series of observations of this comet has been made at the Dunecht Observatory.

6. *Pechüle*, 1880 (*f*).—The last comet of the year was discovered at Copenhagen on December 16, by Dr. Pechüle. It was small, but bright, 1′ in diameter, with a central condensation. According to the elements calculated by Dr. Holetschek, the comet was in perihelion on November 10. It has been very generally observed, and most of the results are given in recent numbers of the *Astronomische Nachrichten*.

In addition to these six comets, an object was observed by Mr. Swift, at Rochester, N.Y., on August 11, in *Ursa Major*, in R.A. $11^h\ 28^m$ and N.P.D. 22°, and supposed by him to be a faint comet. No motion was detected in one hour. It was lost through cloudy weather on the days succeeding the day of observation, and has not been again seen. E. D.

New Planetary Nebulæ.

The comparatively speaking small number of these objects has been increased by three new ones found during the past year. Professor Pickering has instituted a systematic search for celestial objects with a gaseous or otherwise remarkable spectrum, using a telescope with a direct-vision prism inserted between the object-glass and the eyepiece. This search has been rewarded by the finding of three very small and stellar-looking planetary nebulæ, the positions of which for 1880 are:—

R. A.	$18^h\ 4^m\ 19^s$	Dec. $-28°\ 12'$
	$18\ 14\ 23$	$-26\ 53$
	$18\ 25\ 10$	$-25\ 13$

About an eighth of the visible heavens has so far been explored at the Harvard College Observatory, but no other monochromatic objects have been found; while, on the other hand, some remarkable star-spectra have been met with.

Using Prof. Pickering's method of search Dr. Copeland has succeeded in finding a fourth new planetary nebula in

R. A. $21^h\ 2^m{\cdot}2$, Dec. $+47°\ 22'$.

This object is remarkable for having, in addition to the three usual nebular lines, a fourth line at W. L. 4694, which probably is the same as the one seen by Dr. Huggins in G. C. 4964. This nebula is, however, not new, but was found by Mr. Burnham in 1873.

In this connection it also deserves notice that Schmidt's new star in *Cygnus*, which, in the autumn of 1877, was found by Dr. Copeland to have become monochromatic, is now by Professor Pickering announced to have lost this peculiarity, and to show a continuous spectrum. J. L. E. D.

Ceraski's new Variable Star.

The star B.D.+81° No. 25 was, on June 23, 1880, discovered to be variable by M. Ceraski of the Moscow Observatory. Further observation proved it to be a remarkable variable star of the Algol type, and a period of 2·492886 days was assigned to it by Prof. Julius Schmidt of Athens, from minima observed by him in August and October (*Astronomische Nachrichten*, No. 2346). More recently (*Astronomische Nachrichten*, No. 2358), from a discussion of fourteen minima observed between August 2 and December 12, Prof. Schmidt has been led to infer a gradual increase in the period, each period being longer than the preceding by $5^s{\cdot}25$. For the prediction of future minima Prof. Schmidt gives the following elements:—

1880, Dec. $7^d\ 10^h\ 6^m{\cdot}7$ Athens M. T. $+2^d\ 11^h\ 50^m{\cdot}812$ E,

where E is the number of periods elapsed. A further correction of $0^{m}\cdot08753$ must be added for the increase of the length of each succeeding period.

The star appears to go through its changes in about six hours, for two hours of which it remains at minimum, with little, if any, variation of light. Its range of magnitude is from $7^{m}\cdot2$ to $9^{m}\cdot3$, and its colour, which, when it is at full light, is a bright bluish-white, changes to a decided orange as it falls to minimum. Two small companions of the 12 and 13 mag. have been measured in position and distance by Dr. Copeland with the Dun Echt Equatoreal with the following results: 12 mag., Pos. 59°·3, Dist. 14″·9; 13 mag. Pos. 323°·7, Dist. 22″·1. G. K.

Mr. Lewis Boss's Declinations of the Fixed Stars.

The United States Northern Boundary Commission has recently published a very important catalogue of the Standard Declinations of 500 Stars, compiled and prepared by Mr. Lewis Boss, the Director of the Dudley Observatory. The catalogue, which originally was intended for the latitude work of the above Survey, became considerably enlarged in its preparation, so as to include a complete investigation and discussion of all modern original catalogues, in order to reconcile the systematic discordances found to exist among independent declination determinations of the highest rank. The work has been carried out in a very thorough and satisfactory manner, and the care exercised in the selection of stars, in the discussion of the various catalogues, and in the reduction of places to the mean epoch 1875·0, renders Mr. Boss's results one of the most important contributions to the subject which astronomy has received.

Mr. Boss assumes, and probably correctly assumes, that the interval of time between the group of early determinations of Bessel (1821), Struve (1824), and Argelander (1830), and the later ones at Leiden, Melbourne, Greenwich, and Washington, is quite sufficient for an independent judgment as to the approximate accuracy and consequent weight of Bradley's results, and that a reliable system of corrections to the various catalogues may be founded on a discussion of recent catalogues alone, taking as the earliest that of Bessel for the mean epoch 1821.

The successive steps in the formation of the catalogue are:

1. The selection of stars, and particularly of a list of stars most frequently observed, to serve as the basis of the normal system, and as a medium of comparison between the various series of observations.

2. The formation of approximate positions and proper motions for the epoch 1875·0.

3. The computation of precession coefficients, and the reduc-

tion of the individual declinations to the required epochs, in order that the assumed declinations may be corrected by comparison with the observed values.

4. The selection of catalogues and series of observations, and the division into three classes.

5. The application to the declinations of these catalogues of certain corrections derived from inspection of the constants and methods of reduction.

6. The collection of results and formation of the approximate normal system from a discussion of the declinations of the fundamental and principal circumpolar stars, using observations only of sufficient independence and weight.

7. By the aid of the approximate corrections, to enlarge the number of standard declinations, and with these to derive the systematic corrections to the *Fundamenta*.

8. To ascertain corrections to the assumed declination and proper motion of each star, and then to compute definitive corrections and weights for all the catalogues, except those of the third class, with which final weights and corrections the definitive declinations are computed as they appear in the catalogue.

9. The computation of systematic corrections to catalogues of the third class.

The principles adopted for the selection of the stars divide them into five classes:

1. The fundamental and principal circumpolar stars which have been by common consent universally observed.

2. A class of stars less frequently observed, but with the observations so distributed in time that reliable determinations of declination and proper motion can be had without recourse to Bradley's observations; and which, together with the fundamental stars, may serve to construct an approximate normal system for the epoch 1755.

3. A class of stars similar to the last, but lacking in satisfactory authorities for the epochs included between 1820 and 1840.

4. A considerable number of stars which do not furnish proper material for ascertaining systematic corrections to the principal authorities, but valuable for the purpose of perfecting the system of corrections adopted for a few catalogues of small weight.

5. The remaining declinations are such as depend on few authorities, practically of no service in ascertaining systematic corrections.

Nearly all authorities for declination have been selected by the author for use. A careful examination and discussion of each catalogue is furnished for the purpose of applying such corrections as may appear advisable from inspection. These corrections may be regarded as of three classes:—

First, those required on account of the reduction of the observations from apparent place to the true epoch of the cata-

logue, principally for nutation and the proper motion employed.

Second, corrections derived by special examination of the instrument or discussion of the observations which had been indicated but not applied in the results, including *errata*.

Third, corrections applied to certain annual catalogues to reduce the discordances in the results of separate years.

Under the designation of each catalogue are enumerated all the corrections which have actually been applied in the discussion. The investigation of original catalogues which follows will be found especially valuable. (It is unfortunate the author should have introduced a new method of designation for different catalogues, formed of the initial and final letters of the English names of the place where the catalogues were made. This gives some trouble in identification, especially to foreign astronomers, which might have been avoided.)

In considering the Greenwich catalogues, Mr. Boss discusses the R−D correction, and in this connection it is to be regretted that Mr. Christie's memoir on the Systematic Errors of the Greenwich North Polar Distances (*Memoirs of the Royal Astronomical Society*, vol. xlv.) was not published early enough for his use. From a discussion of approximate results, Mr. Boss adopts a mean residual flexure of $+0''.21$, and states that if the uniform value of sine flexure $+0''.21$ had been employed throughout the series, we should have had sufficiently good agreement between direct and reflection observations. He adds that this appears to throw discredit upon the value of flexure derived from opposing collimators. Mr. Christie states that the old law $a+b \sin z$ on the whole represents the observations better than the assumed law $a+b \sin z \cos^2 z$, and adds that "this conclusion if accepted would imply that the determination of flexure correction is erroneous."

In the assignment of weights, which Mr. Boss discusses in Section VI., in the first approximation to a normal system, he says, there is great difference in the quality of independent determinations of declination, which vary from such special investigations as those of Bessel (1821), Struve (1824), Argelander (Abo), and Pulkowa (1845)—where every precaution has been exercised to remedy the inevitable defects of instruments by variety in the circumstances of observation, and the utmost skill and rigour in the computations, with determinations of the various elements of atmospheric refraction—to those which assume their refractions from alien authority, which are made with non-reversible instruments and with little attention to those details of observation and reduction so essential in the delicate problem of measuring absolute declinations; and he justly says that it seems but reasonable to suppose that declinations of the former class are entitled to much greater confidence than those of the latter. He further adds that the weights assigned are not those which might seem to be required by comparisons with an ap-

proximate mean system, but are derived from an independent study of each catalogue, considered solely on the basis of internal evidence. He assigns very low weights to Greenwich catalogues compared with Pulkowa, Leiden, Bessel (1821), Struve (1824), and Argelander (Abo), but the final results show that the declinations of the Greenwich catalogues are quite as good as the others.

In Section VIII., Mr. Boss discusses the final corrections and weights, in which his examination of the systematic corrections and probable errors of each catalogue will be found most exhaustive.

The resulting catalogue of 500 stars comprises stars down to the 6·5 mag. in both hemispheres. The approximate R.A.'s are given, adopting, where possible, Professor Newcomb's standard R.A.'s and his corrections to the R.A.'s of other catalogues; the annual variation and secular variation in R.A. are appended. Accompanying the definitive declinations, Mr. Boss has usefully given columns containing the probable error and weight; the remaining columns are devoted to the annual variation with its probable error, secular variation and third term of precession, and proper motion.

The amount of labour and care Mr. Boss has bestowed on this important work, and the ability he has displayed in the discussion, entitle him justly to the gratitude of all astronomers.

E. B. K.

Professor Safford's Catalogues of the Declinations of Stars.

In the year 1873 the United States Lake Survey published an important Catalogue of the Declinations of 981 Stars for the latitude work of that Survey. This catalogue was prepared by Professor T. H. Safford, the stars ranging down to the 7·0 mag. and situated in R.A. 0^h to 2^h and 12^h to 24^h, and between the parallels of $+30°$ and $+60°$, reduced to the epoch 1875·0. The places were deduced from most modern catalogues. For stars which were not contained in Mädler's Bradley or the catalogue of the Astronomische Gesellschaft, Professor Safford himself has deduced proper motions by comparison with D'Agelet, Fedorenko, Lalande, Piazzi, Groombridge, and a few others. Very few stars are contained in the catalogue which are not derived from two original catalogues. The R.A., with its annual variation and proper motion, is given, and to the Declinations, which are calculated to $0''{\cdot}1$, are appended the annual precession, proper motion, and logs. a', b', c', d'.

During the past year the Engineer Department of the United States army has published a more extensive and valuable catalogue by Professor Safford, being one of the mean declinations of 2018 stars from 0^h to 2^h and 12^h to 24^h Right Ascension, and 10° to 70° of North Declination for January 1, 1875. The stars are situated in those hours of R.A. most suitable for ob-

servation during the months of the year best adapted to the work of the United States Geographical Surveys.

It will be seen that this catalogue is an extension of the previous one of 981 stars, but there are certain differences which render it the more valuable. In this catalogue proper motion is included where given in the final catalogue; in that of the Lake Survey, only where the star is determined by Mädler. In the latter catalogue systematic corrections were used in part; but in that of 2018 stars they are employed and have been applied in the details, which was not so in the first catalogue. Professor Safford states that he has found it necessary to classify the stars according to the degree of accuracy with which their proper motions have been determined.

The stars are divided into four classes, as follows:—

AA. Those whose positions rest upon a great multitude of recent observations.

A. Those whose places have been well settled by at least three observations at each of two Observatories, or by a greater number of observations at one since 1860, or since 1855 when the older determinations give the proper motion with great certainty.

B. Those whose places have been but once so fixed since the same dates.

C. Those stars which had not been well observed by the older astronomers, Bradley, Piazzi, Lalande, and Groombridge, and which have not been lately observed. These stars are classed together for two reasons: first, that the observers at fixed Observatories may see at a glance what stars especially need redetermination in both elements; and second, that these stars may be used in latitude work only in case of absolute necessity.

The author's estimate of the probable error of the Declination for 1875·0 of an average star in each of these four classes is as follows:—

Class AA	±0″·18
„ A	±0″·28
„ B	±0″·43
„ C	±0″·7

In the preparation of the catalogue, Professor Safford has had the advantage of using Professor Auwers's re-reduction of Bradley, and the current Greenwich, Oxford, and Washington observations a year in advance of publication. The author particularly remarks upon having found some scattered and isolated determinations of Declination in connection with latitude work by Gauss, Argelander, Struve, Bessel, and others, which have proved of the highest precision. All modern catalogues have been consulted. Bessel's precessions have been used; for proper motion, the author employs Mädler's values (or an approximation of his own) in R.A., and in Declination he determined the value adopted by comparison of modern observations

with ancient. In combining positions, the following weights are used:—

1 Observation	. . .	Weight	$\frac{1}{2}$
2 „	. . .	„	$\frac{3}{4}$
3 to 8 „	. . .	„	1
9 „	or more .	„	$1\frac{1}{2}$ or 2

Professor Safford states that "the revision of the present catalogue by new observations need only be made for Class C;" and he goes on to make the following significant remarks: "I have been convinced by the present discussion that the ordinary meridian work of Observatories needs to be modified, if the accumulation of raw material, undiscussed and rendering discussion more and more difficult, is not to go on for an indefinite future. The subjects of stellar meridian observation in the northern hemisphere of the heavens now needing attention are these:—

"1. More thoroughly good fundamental determinations.

"2. The observation (aside from the zones) of those of Heis's stars which are not in the B.A.C.—about 2000 in number; these stars are as much needed as those in the B.A.C.

"3. The revision of doubtful cases, such as those in Class C of the present work.

"4. The regular observation every few years of stars of decided proper motion.

"Any other stellar observations need be only for special practical purposes."

These remarks are well worth the consideration of astronomers; likewise Professor Safford's concluding words: "I may remark that experience has shown the importance of keeping a continual watch over predicted star-positions, owing in part to the uncertainty of the older observations and the consequent difficulty of the prediction; it will therefore be well if any suspected stars are immediately made known to me, in order that any discrepancies may be detected by the newer observations now published and in progress." The catalogue includes some stars of the 8th mag. Annual precession and proper motion are given in both elements, and for Declination logs. a', b', c', d', are appended, the details of positions from which the definitive Declinations have been obtained being also given for every star. E. B. K.

The Parallax of α Centauri.

The large value of the parallax of *α Centauri* which Henderson deduced from his meridional observations made at the Cape in 1832–33, after Johnson had called attention to the large proper motion of this splendid double star, was substantially confirmed by the series of observations made by and under Maclear from

1839 to 1849, from which he derived $0''.917 \pm 0''.030$ as the resulting value of the parallax. And it was further supported by some series of observations made by Moesta at Santiago from 1860 to 1864, which give the value $0''.880 \pm 0''.068$ (*Astron. Nachr.* No. 1868). Both determinations depend on the measured zenith distances of the star, and are consequently subject to some sources of error, the effect of which, in mere differential observations, would be greatly diminished. From similar series of observations of the star *β Centauri*, which differs in declination only 33′ from *α* and is only 4° 33′ distant, Maclear and Moesta have likewise determined its parallax and have deduced the values—

Maclear	$0''.470 \pm 0''.044$.
Moesta	$0''.213 \pm 0''.069$.

As the annual proper motion of *β Centauri* does not exceed $0''.1$, such a considerable parallax of the star is at least not very probable, and it becomes a question whether the deduced values do not rather represent the effect of periodical errors in the observations than the parallax itself. Since such periodical errors will be in great part eliminated in the observed differences of the observed zenith distances of *α* and *β Centauri*, the difference of the parallaxes of the two stars can be determined with far greater certainty than the parallaxes themselves, and, at the suggestion of Professor Winnecke, Mr. W. Elkin has, therefore, undertaken such a determination. On account of the rapid revolution of the two components of *α Centauri* round their common centre of gravity and the consequent variability of their proper motions, some investigations had first to be made for ascertaining their mutual position and the proper motion of their centre of gravity. The elements of the orbit of the binary star, which Elkin has deduced from the observed position-angles, are:

Time of periastre . .	1875·97	Periastre from node	54°·79	1860·0
Period	77ʸ·42	Node . . .	25·76	
Eccentricity . . .	0·5100	Inclination . .	79·52	

and the semi-axis from the observed distances $17''.50$. These elements represent the micrometrical measurements within plausible limits, and are sufficiently near the truth for the intended purpose. Elkin's investigation of the proper motion of *α* relative to *β Centauri*, from a discussion of the mean places of the stars in the available star-catalogues, leaves the question undecided whether the centre of gravity of the two components of *α Centauri* differs from the middle point between them, and, consequently, whether their masses are not equal—a curious result, when the difference in their magnitudes is considered. The absolute proper motion of the middle point is found to be approximately $3''.669$ in the direction of the position-angle 281°·75 for 1860.

All three stars α_1, α_2, and β *Centauri* have been observed by Maclear on the same day on 95 occasions, and these observations have then served for the determination of the difference of their parallaxes. The deduced values are:

Relative parallax of α_1 . $0''{\cdot}6014 \pm 0''{\cdot}0813$

„ „ α_2 . $0''{\cdot}4234 \pm 0''{\cdot}0899$;

the simple mean of which,

$0''{\cdot}5124 \pm 0''{\cdot}0797$,

is proposed by Elkin to be accepted as the final result of Maclear's observations.

Moesta's observations of α_2 and β *Centauri* give a relative parallax of $0''{\cdot}369$, but indicate also a large correction of the constant of aberration. Elkin's remarks have induced Moesta to publish in No. 2355–56 of the *Astron. Nachr.* explanations and corrections, and to deduce, himself, from his observations, the difference of the parallaxes $0''{\cdot}348$. Further investigations will be looked for with interest. A. M.

Mr. G. H. Darwin's Researches on the History of the Solar System.

In his previous papers, Mr. Darwin has considered the effects which tidal friction must have had upon the motions of the Earth and Moon, on the supposition that sufficient time has elapsed to allow this agency to produce its full effect. These papers were briefly noticed in the Annual Report of last year (pp. 240–243), and it was there mentioned that the Earth and Moon were traced back to a time when the day was reduced to six hours, the sidereal month to twelve hours, and the obliquity of the ecliptic to 14°. In a memoir "On the Tidal Friction of a Planet attended by several Satellites, and on the Evolution of the Solar System" recently communicated to the Royal Society, but which is not yet published, Mr. Darwin has considered the more general question of the part which tidal friction must have played in the evolution of the solar system as a whole, and of its several parts. The author concludes that the orbits of the planets round the Sun can hardly have undergone a sensible enlargement from tidal friction since these bodies attained a separate existence, and that, with regard to the sub-systems, although it is possible that the orbits of the satellites of *Mars*, *Jupiter* and *Saturn* about their planets may have been considerably enlarged, yet it is certainly not possible to trace the satellites back to an origin almost in contact with the present surfaces of their planets, in the same manner as was done in the case of the Moon in the paper referred to above. There are several peculiarities which distinguish the Earth and Moon from the other sub-systems, so that this result is not to be regarded as unfavourable to the hypothesis

of tidal friction. It seems to indicate, however, that some important cause of change besides tidal friction has been concerned in the evolution of the solar system, and according to the nebular hypothesis this cause would be the condensation of the heavenly bodies. The author therefore proceeds to consider the manner in which contraction and tidal friction are likely to have worked together. It is shown to be probable that solar tidal friction was a more important cause of change when the planets were less condensed than it is at present; and that if a planetary mass generates a large satellite, the planetary rotation is reduced after the change more rapidly than before, but that, nevertheless, the genesis of such a satellite is preservative of the moment of momentum which is internal to the planetary sub-system. This conclusion is illustrated by the comparatively slow rotation of the Earth, and by the large amount of angular momentum residing in the system of the Earth and Moon. The author is led, by considering the distances of the various planets from the Sun, to suggest the following cause for the observed distribution of satellites:

"According to the nebular hypothesis a planetary mass contracts, and rotates quicker as it contracts. The rapidity of the revolution causes its form to become unstable, or perhaps, as seems more probable, an equatoreal belt gradually detaches itself; it is immaterial which of these two really takes place. In either case the separation of that part of the mass, which before the change had the greatest angular momentum, permits the central portion to resume a planetary shape. The contraction and increase of rotation proceed continually until another portion is detached, and so on. There thus recur at intervals a series of epochs of instability or of abnormal change. Now, tidal friction must diminish the rate of increase of rotation due to contraction, and therefore, if tidal friction and contraction are at work together, the epochs of instability must recur more rarely than if contraction acted alone.

"If the tidal retardation is sufficiently great, the increase of rotation due to contraction will be so far counteracted as never to permit an epoch of instability to occur. Now, the rate of solar tidal friction decreases rapidly as we recede from the Sun, and therefore these considerations accord with what we observe in the solar system. For *Mercury* and *Venus* have no satellites, and there is a progressive increase in the number of satellites as we recede from the Sun.

"Whether this be the true cause of the observed distribution of satellites among the planets or not, it is remarkable that the same cause also affords an explanation of that difference between the Earth with the Moon and the other planets with their satellites, which has permitted tidal friction to be the principal agent of change with the former but not with the latter. In the case of the contracting terrestrial mass we may suppose that there was for a long time nearly a balance between the retardation due

to solar tidal friction and the acceleration due to contraction, and that it was not until the planetary mass had contracted to nearly its present dimensions that an epoch of instability could occur. If the contraction of the planetary mass be almost completed before the genesis of the satellite, tidal friction, due jointly to the satellite and the Sun, will thereafter be the great cause of change in the system, and thus the hypothesis that it is the sole cause of change will give an approximately accurate explanation of the motion of the planet and satellite at any subsequent time. It is shown in the previous papers of this series that this condition is fulfilled with the Earth and Moon."

The author considers that the series of investigations he has undertaken afford no grounds for the rejection of the nebular hypothesis, but on the contrary give evidence in favour of its main outlines, although they show that tidal friction must have introduced modifications of great, and in the case of the Earth and Moon paramount, importance.

The memoir also contains a mathematical investigation of the changes produced by tidal friction in a system consisting of a planet attended by any number of satellites revolving in circular orbits.

The Libration of the Moon.

Since Wichmann essayed to determine the Moon's physical libration from his own heliometrical measurements, more than thirty years have been allowed to elapse before further advances have been made in the practical solution of this interesting problem. Wichmann's investigation was founded upon fifty sets of observations of the crater Moesting A, referred to seven points of the Moon's illuminated limb. The discussion established the smallness of the physical libration, at least of the terms of not very long periods, and rendered it probable that their greatest effect in shifting the apparent position of a lunar spot, as seen from the earth, does not exceed two, or at the most three, seconds of arc. As this effect is, moreover, masked by the effects produced by some troublesome sources of uncertainty, it is easily understood that the real determination of the several terms of the physical libration can only be hoped for from improved methods of observation. A valuable contribution towards such a determination has now been made by Dr. E. Hartwig at Strassburg. Though the heliometer employed in the measurements is considerably smaller than the Koenigsberg heliometer, it has the advantage of some improvements in the construction of the measuring apparatus; and Hartwig has been enabled to obtain results which appear to be considerably less uncertain than those of Wichmann. The investigation is founded upon forty-two sets of observations, referring to the same crater, and

extending from November 1877 to January 1879. The deduced inclination of the lunar equator to the ecliptic is 1° 36′ 34″, with p. e. 113″·0, while Wichmann has 1° 32′ 9″, with p. e. 249″·5; so that the difference of 4′ 25″ is within probable errors. An earlier value, deduced by Kreil from observations made 1833–34 at Milan, is 1° 35′ 48″. The selenographical places of the crater agree, as was to be expected, better in latitude than in longitude. The values of the chief term of the physical libration in longitude approach sufficiently to render its fair determination probable. A renewed, or partly renewed, reduction of Wichmann's observations, and the reduction of the long series of Schlüter's observations, made 1841–44, will furnish data for investigating the terms of long periods. Since, moreover, the results of the measurements of the photographs taken at the Oxford University Observatory will be published before long, there is a prospect of some decided progress in our knowledge of the Moon's libration. A. M.

The Photometric Observations of the Harvard College Observatory.

These observations, made principally with the Equatoreal Telescope of 15-inches' aperture, during the years 1877–79, have been published in vol. xi., parts 1 and 2, of the Annals of that Observatory. In Part I., Professor Pickering commences with a description of the various forms of photometer employed, most of which, however, depend upon the combination of a double-image prism and a Nicol prism, so that two images of each of two bright objects in the same field were formed, either of which by turning the Nicol could be made as faint as desirable. Thus the faint image of the brighter object could be reduced to equality with the bright image of the fainter, and the relative brightness would be then deduced from the angle through which the Nicol is turned. On this principle the author remarks, "All of the photometers described are open to the objection that the loss of light is very great. Under the most favourable circumstances, only ·20 to ·40 of the light is used; so that with the large telescope of 15 inches' aperture, faint objects appear no brighter than with a telescope having an aperture of 7 to 9½ inches, with a common eyepiece." To meet this difficulty, photometers of different construction were used, the faint object being viewed directly with an eyepiece of the usual form, and compared with some bright object, assumed as a standard, the light of which, passing through an auxiliary telescope, was reflected into the field, and reduced by a known amount until it was no brighter than the object to be measured. The reduction of the observations with this class of photometer depends, however, upon the relative amounts of light transmitted by the large and small

telescopes. In the auxiliary telescope the light was reduced either by the introduction of a diaphragm with a minute hole in it, as in Photometer D, or the aperture was varied by a screw moving two sliding V-shaped pieces of brass, as in Photometers E, G, I, E′, and J.

In Part II., the constants of these photometers are discussed. With regard to Photometer D, the constant was determined—

1. By observation of planets through minute holes.
2. By observation of artificial stars.
3. From the light of clouds.
4. From the satellites of *Saturn*.

Considering the difficulty and the necessary uncertainty in such observations, particularly where the measurement of the aperture of a very minute hole is involved, the collected results are very accordant. The same applies to the discussion of the constants of Photometers E, G, I, and J, with this difference, that as in these latter photometers an accurate value of the constant could be obtained by computation, the author, finding that his observations were fairly accordant with the theoretical value, adopts it for reducing the observations.

Part I. gives a journal of over 5,000 observations made from August 1877 to March 1879, which comprise over 25,000 settings.

The conjunctions of the planets *Saturn* and *Mars*, and *Jupiter* and *Venus* in 1877, afforded a good opportunity for comparing their relative brightness. From 600 comparisons of *Saturn* and *Mars* made on 25 nights, and from 300 of *Jupiter* and *Venus* on 11 nights, with photometers without a telescope, the author finds—

Albedo of *Saturn* = 4·62 × Albedo of *Mars*,
„ *Jupiter* = 0·86 × „ *Venus*.

This is on the assumption of the mean diameter of *Mars* being 10″·10. If Le Verrier's value, 11″·10, be adopted, the result will be 5·59 × albedo of *Mars*, or if Kaiser's value of 9″·4 be taken, then 4·01 × albedo of *Mars*, will be the correct expression for the albedo of *Saturn*.

A series of photometric observations of all the more conspicuous double stars visible in the latitude of Cambridge, U.S., was made. The greater portion of this series was made with Photometer H, specially adapted for comparing objects very near together. The observations are given in a ledger, and the concluded results in a catalogue of 190 bright double stars. The examination of these results shows in the majority of cases such good accordance that they must carry a very high weight. Professor Pickering gives a very complete discussion of the effects of distance, position-angle, focus, and proximity of bright stars, which makes this portion of the work remarkably valuable and interesting.

Professor Pickering records an observation of the comparative brightness of *Titan*, situated very near the limb of *Saturn*, and *Rhea* following it at some distance. To the eye these satellites

appeared nearly equal, but from measurements with Photometer H, *Titan* was found to be 1·4 mag. brighter than *Rhea*. This is rather less difference than has been usually adopted.

A series of measurements was made on four nights to show what accuracy was to be expected in determining the light of a star by its limit of visibility. Four stars were selected for the purpose—*a Ursæ Minoris*, *Cephei* 51, λ *Ursæ Minoris*, and B.D. 89°, 3. The images of *a Ursæ Minoris* could not be made to disappear. Professor Pickering states that the residuals show that a variation of several magnitudes is to be expected in the observations by different persons on different days, and even when the results are rendered differential, the variations are so large that the results have little value. It is noticeable, however, that the observations of observer U, whose results were obtained on the two intermediate nights, are discordant *inter se* and with the others, whereas the results of observer P on the first and last nights are fairly accordant, and do not support the above statement.

In chapter vii., Professor Pickering discusses his investigation of the magnitudes of the satellites of *Mars*. The methods he adopted show great ingenuity in overcoming so many difficulties as naturally present themselves in determining the brightness of such minute points close to a brilliant primary. As the author states, "to measure the brightness of these satellites in terms of their primary involved the determination of a photometric interval as great as that between the Sun and Moon. The results obtained in 1877 can fortunately be compared with a second series of measurements made in 1879, which are given in an appendix; and the general conclusion arrived at is that *Deimos* is about 14·5 magnitudes fainter than *Mars*, and about 0·3 mag. fainter than *Phobos*.

An interesting discussion of the dimensions of these satellites, derived from photometric observations, is given, the equivalent diameter of the satellite being calculated on the assumption that its albedo is the same as that of *Mars*. The results on this assumption must of course be accepted with caution.

The observations of the satellites of *Jupiter*, in chapter viii., are of especial interest from the comparison of the equivalent diameters (photometrically inferred) and the diameters as actually measured. The following are the results:—

Satellite	I.	II.	III.	IV.
Light ratio	1786	2032	1271	3597
Equivalent diameter . .	0″·873	0″·818	1″·035	0″·615
Measured „ . .	1″·081	0″·910	1″·537	1″·282
Relative albedo . . .	0·652	0·809	0·453	0·230

The first line gives the ratio of the light of *Jupiter* to that of each of the satellites, the last the albedo relative to *Jupiter*. To obtain the absolute albedo for each satellite, the relative albedo

must be multiplied by the albedo of *Jupiter* (Engelmann's value 0·610). Professor Pickering's results show that Satellite I. is brighter than Satellite II.; but in this connection it is interesting to compare Mr. Common's photograph of *Jupiter* and his satellites, in which II. appears decidedly brighter than I. Professor Pickering adds that the view that the light of the satellites is variable, is not sustained by his observations.

A similarly complete discussion of the relative brightness of the satellites of *Saturn* is given in chapter ix., followed by observations of the two outer satellites of *Uranus* and the satellite of *Neptune*.

We cannot speak too highly of the energy and ability that Professor Pickering has shown in carrying out so successfully this most important photometric work—a research that redounds so very much to the honour and credit of the Harvard College Observatory. It is gratifying to know that the further photometric work referred to in the Council's Report last year is being carried forward with equal energy. We learn by Professor Pickering's recent Annual Report that the work with the Meridian Photometer of measuring all stars visible to the naked eye between the North Pole and $-30°$, has already been more than half accomplished; and it is hoped that it may all be completed by October next. The number of separate settings made is as follows:—30,076 of ordinary stars, 2,996 upper culminations, 3,168 lower culminations, 1,268 of bright stars, and 2,848 of the Pole Star; total, 40,356. The photometer, it will be remembered, consists of a transit instrument in which polarised images of the star to be measured, and of the Pole Star, are placed side by side in the field, and brought to equality by turning a Nicol prism inserted in the eyepiece. The instrument is so constructed that the two objects are viewed under precisely the same conditions, with the same magnifying power, the same aperture, the same background, and the same emergent pencil. Moreover, their positions are reversed during each observation. The observer remains in comparative darkness, and confines his attention to the settings, all the readings of the circles and the recording being done by an assistant.

We are informed that the principal want of the Observatory at the present time is means for the publication of its Annals, but we sincerely trust funds may be found available for the publication of the very important work foreshadowed above. E. B. K.

Photographic Star Spectra.

Dr. Huggins has published in greater detail than before (*Phil. Trans.* clxxi. [ii.] p. 669, 1880) his researches on the photographic spectra of stars. The stars may be divided into groups.

Some of them, including the principal white stars, give a spectrum of twelve very strong lines, probably all belonging to hydrogen. The second group gives spectra resembling that of our Sun. The third includes the red stars; the spectrum is crowded with lines which appear broader than those in the solar spectrum. The groups, however, are not well defined, and gradually fuse into each other. This is best shown by the gradual appearance of the calcium line K. As Dr. Huggins remarks, "The spectra of these stars may be arranged in a continuous series, in which first we find this line to be absent. Then it appears as an exceedingly thin line. We then pass into another stage in which it is distinct and defined at the edges; in the solar spectrum it becomes broad and winged; and, lastly, in *Arcturus*, there is further progress in the same direction, and the line, now a broad band, exceeds in intensity H." The following is the order of the stars photographed by Dr. Huggins, when arranged in the manner indicated: *Sirius, Vega, η Ursæ Majoris, α Virginis, α Aquilæ, Rigel, α Cygni, Capella, The Sun, Arcturus, Aldebaran, Betelgeux.* Dr. Henry Draper has also succeeded in obtaining photographs of some star spectra. A. S.

Solar Physics.

The increased number of Sun-spots observed in the last year has no doubt renewed the activity of solar physicists, but not much of the work has as yet been published. Professor Young has examined the solar spectrum under high dispersion, and paid special attention to the lines, which appear in Ångström's map to be common to two substances. Amongst 70 lines thus examined 56 have been distinctly seen double or triple.

Particular interest attaches to the lines H and K of the solar spectrum, on account of their remarkable behaviour in the spectra of stars as photographed by Dr. Huggins. These lines have hitherto been attributed to calcium, but H seems also to be coincident with a hydrogen line. Professor Young has observed in the chromosphere H to be always accompanied by a fine bright companion. This companion he at first thought to be the hydrogen line; but a remarkable prominence observed by him caused him to modify this opinion and to offer the suggestion that both H and K are due to hydrogen.

A striking spectrum of a Sun-spot was observed at Greenwich at the end of November. A number of strong lines as broad as b_1 or b_2 appeared between b and F. None of these lines are found on Ångström's or Kirchhoff's map, but under favourable circumstances faint lines coinciding with them could be seen in the ordinary solar spectrum.

Mr. Lockyer has in two papers communicated to the Royal Society his observations on the distortion of lines in Sun-spots.

Some of the lines of iron were strongly distorted, while others remained perfectly clear and straight.

Professor Tacchini continues his observations on Sun-spots and solar prominences, and is accumulating material which no doubt will ultimately prove to be of great importance.

A. S.

Captain Abney's Map of the Infra-Red End of the Solar Spectrum.

Captain Abney has published his map of the infra-red parts of the solar spectrum as photographed by him.* The spectrum could be traced in the prismatic spectrum as far as wave-length 12000 tenth-metres. It is somewhat shorter in the diffraction spectrum. The least refrangible part, however, is perfectly continuous, the last absorption line lying about 9825. There seem to be comparatively few sharp lines in this part of the spectrum, but chiefly bands. Captain Abney could not, except perhaps in one or two cases, trace any thickening of the lines when the Sun was at low altitudes; so that they do not seem to have an atmospheric origin. A. S.

The Velocity of Light.

A careful determination of this important constant was made by Albert A. Michelson, Master U. S. Navy, in the summer of 1879, and the details of the observations have been recently published as Vol. I. Part III. of the "Astronomical Papers for the Use of the American Nautical Almanac." Mr. Michelson employed Foucault's method of a revolving mirror, with modifications in the optical arrangements which enabled him to obtain a deflection of $133^{m.m.}$, and thus to remove the objection to this method that the displacement is too small to be measured with sufficient accuracy. By using a lens of great focal length, and placing the revolving mirror 15 feet within the focus, he combined a large distance between the mirrors with a considerable radius for the deflection of the image. Thus with a collimating lens of 8 inches' diameter and 150 feet focal length a sufficiently bright image was obtained when the fixed mirror was at a distance of 2000 feet. In Foucault's experiments the greatest distance which it was found practicable to use was only 20 metres. The revolving mirror was driven by an air-turbine, actuated by an engine of 3-horse power with automatic regulator. The speed of rotation was about 257 revolutions in a second, this being measured by means of a tuning-fork, bearing on one prong a steel mirror, which reflected the light received from the revolving

* *Phil. Trans.* vol. clxxi. part 2, p. 653.

mirror to the eye, and gave a stationary image when there was exact synchronism between the vibrations of the fork and the revolutions of the mirror.

Between June 5 and July 2, 1879, 100 sets of observations, each containing ten individual measures were taken, and from these Mr. Michelson deduces, for the velocity of light, the value 299,940±51 kilometres, or 186,380 miles per second. In his estimate of the probable error he has taken into account the probable systematic errors in the measurement of the distances between the mirrors, in the determination of the speed of rotation and in the measurement of the deflection. The probable error as inferred from the casual discordance of the individual results is only 5 kilometres.

Mr. D. P. Todd has, in the *American Journal* for 1880, January, combined Mr. Michelson's determination with those of Cornu and others, giving a weight of 100 to Michelson's, 25 to Cornu's, and 1 to each of the others, and thus deduces for the most probable value of the velocity of light, 299,920 kilometres = 186,360 miles per second. Combining this with the value $498^{s}{\cdot}3$ for the light equation from *Jupiter's* satellites, as inferred from Glasenapp's and Delambre's determinations, he obtains for the Sun's distance 92,866,000 miles, and for the Solar parallax $8''{\cdot}802$. Again, taking Struve's constant of aberration $20''{\cdot}4451$, in combination with the value found for the velocity of light, the Sun's distance would be 92,768,000 miles, and the parallax $8''{\cdot}811$. Mr. Todd estimates the uncertainty in the velocity of light as 70 kilometres, that of the light equation as 1^{s}, and that of the constant of aberration as $0''{\cdot}025$, and thus obtains what he considers limiting values for the several quantities concerned. Combining these he obtains for the extreme values of the Solar parallax deduced from the velocity of light, $8''{\cdot}782$ and $8''{\cdot}824$, and for the final result $8''{\cdot}808\pm0''{\cdot}006$, corresponding to a mean distance of 149,345,000 kilometres = 92,800,000 miles.

Transatlantic Longitudes.

In continuation of the series of telegraphic determinations of longitudes, which have been carried out so completely in the West Indies and Central America, under the auspices of the U.S. Navy Department, Lieutenant-Commander Green and his party of American officers have fixed the longitude of the ports on the east coast of South America by means of the submarine cable from Lisbon to Pernambuco, and have thus established a most important series of secondary meridians of longitude. In particular the determination of the longitude of Buenos Ayres affords an exact starting-point for the extensive chain of meridian distances measured from Cordoba by Dr. Gould—a chain which extends from Asuncion in Paraguay on the north to Buenos

Ayres on the south, and from Colonia on the east to Santiago de Chile on the west.

The work was carried out in the years 1878 and 1879, and the results are given in detail in the Report recently published by the U.S. Navy Department. The chain of longitudes first completed was Lisbon-Madeira-Porto Grande (St. Vincent)-Pernambuco, one observer being placed alternately east and west of the other, so as to eliminate personal equation as far as possible. At Pernambuco it was found that the cable to Rio Janeiro was broken, and this section had to be deferred. Meanwhile the portion Rio Janeiro-Montevideo-Buenos Ayres, was determined, and the expedition returned to the United States. Next year (1879) the difference of longitude Greenwich-Lisbon was determined, Porthcurnow and Carcavellos (the two ends of the submarine cable) being used as transmitting or repeating stations. Subsequently the links Rio de Janeiro-Bahia, Bahia-Pernambuco, and Pará-Pernambuco were completed, and thus the whole chain of longitudes, Greenwich-Lisbon-Madeira-St. Vincent-Pernambuco-Bahia-Rio Janeiro-Monte Video-Buenos Ayres, was finished.

The importance of this determination may be estimated by the uncertainty in the previously accepted longitudes of some of the points. Thus Commander Green finds that the received longitude of Lisbon is $8^s{\cdot}54$ too small, and that the determination of the longitude of Rio de Janeiro by M. Liais is 35^s in error. Admiral Mouchez had, however, from a discussion of all attainable results, obtained a value for this last longitude differing only 2^s from that found by Commander Green.

Commander Green's determinations of longitude depend for the most part on four nights of observation, and the accordance of the separate results is satisfactory. For the observations, a combination of the transit instrument and zenith telescope with axis-view, designed by Mr. J. A. Rogers, was used, and as these instruments only weigh 125 pounds each, they were easily transported. A discordance between the clock error deduced from observations in reversed positions of the instrument, amounting to from 1^s to $1^s{\cdot}2$, was found to be due to flexure of the axis, and from observations of a large number of stars at various distances north and south of the zenith, the correction for flexure,

$$0^s{\cdot}55 \cos z \sec \delta,$$

was deduced.

It is to be remarked that no correction for personal equation has been applied, as it was found, by repeated use of Professor Eastman's personal-equation machine, that the habitual errors of the various observers concerned had all the same sign, and that their respective differences were very small. A small uncertainty depending on the personal equation of Lieutenant-Commander Davis with the Greenwich observers affects the longitude of the Lisbon station, and, to an equal amount, the other positions of the chain.

Professor Bredichin's Researches on the Tails of Comets.

For several years past Prof. Bredichin has been engaged on a discussion of the forms of comets' tails with reference to the hypothesis of a solar repulsive force acting on the cometary matter emitted from the nucleus, and this has now led him to the important conclusion that the tails of comets belong to three types, corresponding respectively to repulsive forces 11, 1·4 and 0·3 (the Sun's gravitating attraction being taken as 1), and composed presumably of three different sorts of matter.

Prof. Bredichin's researches are contained in the *Annales de l'Observatoire de Moscou*, vols. iii.–vii., 1877–1880, and up to the present time he has discussed the observations of the tails of 33 comets, and has calculated the values of the repulsive force $1-\mu$, under the action of which they would have been generated. For this purpose he has employed the approximate formulæ for the motion of a particle emitted from the nucleus and subject to a repulsive force from the Sun, which Bessel gave in the *Astronomische Nachrichten*, Nos. 300, 301 and 302, and which he there applied to the case of Halley's Comet in 1835. Some errors of sign in these formulæ have been corrected by M. Papo (*Astronomische Nachrichten*, No. 1173) and by Professor Bredichin (*Astronomische Nachrichten*, No. 1291). Bessel's formulæ give the co-ordinates of a particle of the tail at any time T after its emission from the nucleus (T being expressed in terms of the Gaussian unit 58·13244 days), and the expansions are carried as far as T^3. This degree of approximation is sufficient in the case of tails of the first type, for which the repulsive force is large, so that a particle takes but a comparatively short time to traverse the whole length of the tail. But with types ii. and iii. the case is different, and Bessel's formula is not sufficiently accurate for the parts of the tail at a considerable distance from the head. Thus for the comet 1874 iii. (Coggia) Prof. Bredichin found by Bessel's formulæ $1-\mu=0{\cdot}88$ near the head, and $=0{\cdot}47$ near the end of the tail, where T exceeded 0·5. He was at first led to assign a physical cause for this diminution in the repulsive force as the particle receded from the head, but he subsequently recognised that the formulæ were in fault. On using the formulæ for hyperbolic motion, given by Prof. Norton in his memoir on Donati's Comet, and subsequently rigorous formulæ deduced by Prof. Joukowsky and himself, he found that a value of $1-\mu=1{\cdot}2$ satisfied the observations of the tail throughout its whole length, and that in the case of the Comet 1860 iii., the value of $1-\mu$ should be 1·36 instead of 0·64, as found by Bessel's formulæ. To estimate the effect of the error in Bessel's approximate formulæ, Prof. Bredichin has taken a comet with perihelion distance 0·5 (being the arithmetic mean of the perihelion distances of all observed comets), and constructs the tails of the three types, taking

$1-\mu=11{\cdot}0$, 1·4 and 0·3 respectively, and with the rigorous formulæ, computing the co-ordinates for values of t up to 30 days. With these co-ordinates he then finds the values given for $1-\mu$ by Bessel's formulæ at different distances from the head.

The results of Prof. Bredichin's calculations are summarised in a table, giving the types to which the tails of 28 comets belong, together with approximate elements of their orbits (*Annales de l'Observatoire de Moscou*, vol. vi. Part I. p. 58). Adding to these five comets, viz., those of 1682, 1680, 1769, 1744, 1843 and 1880, discussed in a subsequent memoir (*Annales*, vol. vii. Part I.), there are in all 34 comets classified, with 17 tails belonging to type i., 17 to type ii., and 13 to type iii. Several of the comets have two tails, belonging, of course, to different types. The comet of 1744 is remarkable for the five tails seen by Chesćaux and Delisle six days after the perihelion passage. For these Prof. Bredichin finds values of $1-\mu$ ranging from 1·0 to 2·4, and he explains this divergence of the tails by supposing that near perihelion the varieties in the substances of the same type would show themselves. The tail of Donati's Comet, type ii., which near perihelion was decomposed into several juxtaposed conoids, presented an analogous case. The tail of the Comet of 1744 before perihelion was of type i.

For his three types Prof. Bredichin adopts:—

Type i.	$1-\mu = 11\,0$	$g = 0{\cdot}15$	or 4500 metres per 1".
ii.	$1-\mu = 1\,3$ or 1·4	$g = 0{\cdot}03$	or 900 „ „
iii.	$1-\mu = 0{\cdot}2$ or 0·3	$g = 0{\cdot}01 \pm$	or 300 „ „

$1-\mu$ being the solar repulsive force, and g the velocity of emission of the particles, deduced from the observed dimensions of the head.

Considering that these three types represent different substances, and adopting Zöllner's hypothesis of a repulsive force due to electricity and inversely proportional to the specific gravity, Professor Bredichin infers that the tails of the three types are composed respectively of hydrogen, carbon, and iron. Other elements of nearly the same atomic weight may replace or be mixed with the carbon or iron of the second or third types, and thus the tail may be spread out in the plane of the orbit, as in the case of Donati's Comet. But the first type of tail composed of hydrogen will always remain separated from the others. As a support to this hypothesis, Professor Bredichin points out that the velocities of emission for the three types are of the order of molecular velocities of gases, though they are somewhat larger in magnitude (*Annales*, vol. v., Part II., p. 137; vol. vi., Part I, p. 59).

Papers read before the Society from February 1880 *to February* 1881.

1880.

Mar. 12. Phenomena of *Jupiter's* Satellites, observed at Mr. E. Crossley's observatory, Bermerside, Halifax. J. Gledhill.

Observations of the Satellites of *Mars*. Prof. Asaph Hall.

Supplementary note on the changes in the error of Hansen's Lunar Tables. W. T. Lynn.

Observations of *Mimas* and of an Occultation of *Rhea*, in 1879, with the 26-in. equatoreal of the United States Naval Observatory, Washington. Prof. E. S. Holden.

Disappearance of *Jupiter's* Satellites in 1883. Rev. S. J. Johnson.

An examination of the double-star measures of the Bedford Catalogue. S. W. Burnham.

Mean heliographic latitude of Sun-spots for the years 1874 to 1879, deduced from the photographs taken at the Royal Observatory, Greenwich. Astronomer Royal.

Observations of the great southern Comet 1880, from Feb. 2 to Feb. 9, made at the Cape of Good Hope. D. Gill.

Observations of the great southern Comet 1880, from Feb. 1 to Feb. 7, made at Monte Video. Rev. S. S. O. Morris.

Observations of the exterior Satellite of *Mars* at the Oxford University Observatory. Prof. C. Pritchard.

On the longitude of the Observatory, Windsor, New South Wales. J. Tebbutt.

On some changes in the markings of *Mars*. N. E. Green.

Note on Mr. Maxwell Hall's paper on the Nebula in the *Pleiades*. C. Wolf.

Note on the spectrum of Mr. Baxendell's new Star in *Canis Minor*. Dr. H. C. Vogel.

Note on the phenomenon known as "Shadow Bands." Dr. R. Copeland.

On the determination of the personal equation of Lunar Observers. E. Neison.

Description of a proposed new uniform pressure Clock. T. Buckney.

Note on the photographic diameter of the Moon. E. Neison.

Observations of the great southern Comet made at Monte Video. Lieut. B. Gwynne.

Account of observations of the total Solar Eclipse of 1878, July 29, made at Denver, Colorado. A. C. Ranyard.

Apr. 9. Note on a precessional Globe. R. H. M. Bosanquet.

Observations of the great southern Comet, 1880, made at the Melbourne Observatory. R. L. J. Ellery.

Observations of the great southern Comet, 1880, made at the Observatory, Sydney. H. C. Russell.

On mean Refractions. E. J. Stone.

410 radiants of Shooting Stars, deduced from observations in Hungary in the years 1871 to 1878, and 80 probable radiants deduced from the 410 radiants. Dr. N. de Konkoly.

On the relative star magnitude of *Mars* in February and March 1880. Lord Lindsay.

On the theoretical value of the acceleration of the Moon's mean motion in longitude produced by the change of eccentricity of the Earth's orbit. Astronomer Royal.

The Nebula in the *Pleiades*. A. A. Common.

Observations of Meteors, July 1879 to March 1880. D. W. Barker.

Rotation period of *Jupiter*. T. D. Brewin.

May 14. On the variability of B. A. C. 2472. J. Tebbutt.

On the longitude of the Observatory, Windsor, New South Wales (supplement). J. Tebbutt.

Notice of the great southern Comet. J. Tebbutt.

Observations of the great southern Comet, 1880. D. W. Barker.

On a photograph of *Jupiter's* spectrum, showing evidence of intrinsic light from that planet. Dr. H. Draper.

On a mechanical attachment for equatoreal mountings to facilitate sweeping in right ascension. D. P. Todd.

Central Solar Eclipses in Great Britain during 1000 years. Rev. S. J. Johnson.

Elements of Schäberle's Comet. J. R. Hind.

The possible ten-month period of variation in latitude. A. M. W. Downing.

Ephemeris for physical observations of *Jupiter*, 1880–81. A. Marth.

On the preparations to be made for observations of the Transit of *Venus*, 1882, Dec. 6. Astronomer Royal.

On the determination of the Solar Parallax by the parallactic inequality in the motion of the Moon. J. Campbell and E. Neison.

Note on the Astronomer Royal's investigation of the theoretical value of the acceleration of the Moon's mean motion. Prof. J. C. Adams.

Investigation of the secular acceleration of the Moon's mean motion, caused by the secular change in the eccentricity of the Earth's Orbit, taking into account terms of the order m^4, but neglecting the eccentricity and inclination of the Moon's Orbit. Prof. J. C. Adams.

Observations of Comet, 1880 b (Schäberle), made at Dun Echt, with the filar micrometer of the 15-inch refractor. Lord Lindsay.

June 11. On a simple method of finding a Meridian Line. Major-Gen. J. Baillie.

On a partially graphic method of computing Solar Eclipses and Occultations of the Moon. Rev. J. Pearson.

Ephemeris for finding the positions of the Satellite of *Neptune*. A. Marth.

Coincidences of Sun-spots and Auroræ in the olden time. Rev. S. J. Johnson.

On the Star No. 804 in the first Greenwich Seven-Year Catalogue. A. Wagner.

The Babylonian Astronomy. Part iii. R. H. M. Bosanquet and Prof. A. H. Sayce.

On the determination of the Solar Parallax from Heliometer Observations of *Mars*, made at Ascension during the opposition of 1877. D. Gill.

On the first results of the researches in connection with the lunar physical libration. Prof. C. Pritchard.

Addition to the Ephemeris for the physical observations of *Jupiter*. A. Marth.

Note on the Nebula near *Merope* (extract from a letter to the Foreign Secretary). W. Tempel.

On the refractive and dispersive powers of various samples of glass. G. L. Lohse.

Note on the constant of lunar parallax. Prof. J. C. Adams.

Addition to a paper entitled "On the theoretical value of the acceleration of the Moon's mean motion in longitude, produced by change of eccentricity of the Earth's Orbit." Sir G. B. Airy.

On the construction of a new form of Micrometer. Prof. C. Pritchard.

On an inexpensive form of Chronograph. Prof. C. Pritchard.

On a method of determining the pressure on the Solar Surface. Prof. E. Wiedemann.

Nov. 12. Elements of Comet Schäberle (1880 ii.). Prof. T. H. Safford.
Southern double stars. H. C. Russell.
Comparison of the observations of the Sun, made at Washington 1866–75, with Hansen's Tables. Dr. C. R. Powalky.
Third Catalogue of micrometrical measures of double stars, made at the Temple Observatory, Rugby. G. M. Seabroke.
The discovery of κ *Pegasi* as a close double star. S. W. Burnham.
The August Perseids. Admiral Sir E. Ommanney.
On the method of least squares. J. W. L. Glaisher.
On the possible performance of an object glass for stargazing. E. Sang.
Observations of Comet I. 1880, made at the Royal Observatory, Cape of Good Hope. D. Gill.
The July Aquariads, 1880. E. F. Sawyer.
Observations of a meteor. Nov. 4th, 1880. C. F. Varley.
A new Nebula. J. Baxendell.
On the annual parallax of the Star P. III. 242. Prof. R. S. Ball.
On the Spectrum of Comet 1880 d. (Hartwig). W. H. M. Christie.
Saturn in October 1880. W. F. Denning.
Note on Hansen's value of the semi-diameter of the Moon. E. Neison.
The motions and varieties of the Jovian Spots. W. F. Denning.
Observations of Ceraski's variable star in *Cepheus*. G. Knott.
Addition to a paper on the method of least squares. J. W. L. Glaisher.
On a new form of "Ghost" Micrometer. C. E. Burton and H. Grubb.
Note on a Photograph of *Jupiter*. A. A. Common.
Note on the Nebulæ near *Merope*. A. A. Common.
Particulars of the mounting of a 3-foot Reflector. A. A. Common.
A determination of the diameter of *Mars* at the mean distance of the Earth from the Sun. A. M. W. Downing.
Cometary observations made at the University Observatory, Oxford. Prof. C. Pritchard.
Observations of the red spot on *Jupiter*. A. V. Nursinga Row.
Note on a phenomenon of *Jupiter's* Satellites. Capt. W. Noble.

On the determination of the coefficient of the parallactic inequality in the expression for the Moon's Longitude. E. J. Stone.

Telegraphic determination of the longitude of Shanghai. Dr. L. S. Little.

Dec. 10. Note on the length of the Pendulum observed by De L'Isle de la Croyère at Archangel, in 1728. Major J. Herschel.

Observations of the great southern Comet, I. 1880, made at Castlemaine, Victoria. Dr. W. Bone.

On the spectrum of a Sun-spot observed at the Royal Observatory, Greenwich. Astronomer Royal.

Ephemeris for finding the positions of the Satellites of *Uranus*, 1881. A. Marth.

Observations of Meteors, Nov. 25–28, 1880. W. F. Denning.

The Meteors of Nov. 14, 1880, observed at Moncalieri. Signor F. Denza.

Note with respect to the rate of motion of gaseous matter projected from the Sun. A. C. Ranyard.

1881. Jan. 14. Further researches on the annual parallax of 61 *Cygni*. Prof. R. S. Ball.

The binary Star *β Delphini*. S. W. Burnham.

Occultation of Stars by the Moon, seen at the Stonyhurst Observatory. Rev. S. J. Perry.

Observations of *Jupiter's* Satellites, made at the Stonyhurst Observatory. Rev. S. J. Perry.

Further notes on the Nebula in the *Pleiades*. Maxwell Hall.

The partial Eclipse of the Sun, Dec. 31, 1880. J. Rand Capron.

Observations of the Solar Eclipse, 1880, Dec. 31, made at the University Observatory, Strassburg. Prof. A. Winnecke.

On Mr. Stone's alterations of Bessel's Refractions. W. H. M. Christie.

Spectroscopic results for the motions of Stars in the line of sight, obtained at the Royal Observatory, Greenwich, IV. Astronomer Royal.

Observations of occultations of Stars by the Moon, of phenomena of *Jupiter's* Satellites, and of the Eclipse of the Sun, 1880, Dec. 31, made at the Royal Observatory, Greenwich. Astronomer Royal.

Remarks on Dr. Hartwig's recent determination of the lunar physical libration. Prof. C. Pritchard.

Eclipse of the Sun, Dec. 31, 1880, observed at the University Observatory, Oxford. Prof. C. Pritchard.

On the apparent conjunctions of the Satellites of *Uranus*, 1881. A. Marth.

On the North Polar distances of the Cape Catalogue for 1840. A. M. W. Downing.

Notes on Dr. H. Draper's photograph of the Nebula in *Orion*. A. C. Ranyard.

Description of an observing seat for an Equatoreal. Prof. G. W. Hough.

Note on the Nebula in the *Pleiades*. H. Pratt.

On some difficulties connected with the determination of the diameter of *Mars*. E. J. Stone.

The Solar Eclipse of 1880, Dec. 31, observed at the Radcliffe Observatory, Oxford. E. J. Stone.

On the bearing of ancient Eclipses, and especially that of Agathocles, on the Moon's motion. Prof. J. Celoria.

List of Public Institutions and of Persons who have contributed to the Library, &c., since the last Anniversary.

Her Majesty's Government in Australia.
Her Majesty's Government in India.
The Lords Commissioners of the Admiralty.
The Board of Trade.
British Association for the Advancement of Science.
British Horological Institute.
Geological Society of London.
Institute of Actuaries.
Meteorological Office.
Meteorological Society.
Photographic Society of Great Britain.
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Boston, Scientific Society.
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Buda-Pesth, Hungarian Academy of Sciences.
Calcutta, Asiatic Society of Bengal.
Cape of Good Hope, Royal Observatory.
Cincinnati Observatory.
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Milan Royal Observatory.
Moncalieri Observatory.
Montpellier, Academy of Sciences.
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Munich, Royal Bavarian Academy of Sciences.
Neuchatel, Society of Natural Sciences.
New York, Astor Library.
Paris, Academy of Sciences.
Paris, Bureau des Longitudes.
Paris, Dépôt Général de la Marine.
Paris, Ecole Polytechnique.
Paris, International Committee of Weights and Measures.
Paris, Mathematical Society of France.
Paris Observatory.
Paris, Philomathic Society of France.
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Philadelphia, Franklin Institute.
Potsdam, Astrophysical Observatory.
Prague Observatory.
Rome, Central Meteorological Office.
Rome, Italian Spectroscopic Society.
Rome, Pontifical Academy dei Lincei.

Rome, Royal Academy dei Lincei.
St. Petersburg, Imperial Academy of Sciences.
San Fernando Observatory.
Sydney, Royal Society of New South Wales.
Tasmania, Royal Society.
Tiflis, Physical Observatory.
Toronto Meteorological Office.
Toulouse, Academy of Sciences.
Turin, Observatory of the Royal University.
Turin, Royal Society of Sciences.
Upsala, Royal Society.
Vienna, Imperial Academy of Sciences.
Vienna, Imperial Observatory.
Washington, American Ephemeris Office.
Washington, American Philosophical Society.
Washington, Smithsonian Institution.
Washington, United States Hydrographic Office.
Washington, United States Naval Observatory.
Editor of the American Journal of Science.
Editor of the American Journal of Mathematics.
Editor of the Analyst.
Editor of the Astronomical Register.
Editor of the Astronomische Nachrichten.
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Editors of Urania.

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Prof. C. V. Zenger.

ADDRESS

Delivered by the President on Presenting the Gold Medal of the Society to Professor Axel Möller.

GENTLEMEN,—

It now becomes my duty to put before you the main points in the investigations of Professor Axel Möller on the motion of Faye's Comet, for which your Council have this year awarded him the Gold Medal of the Society, and this I shall endeavour to do in as succinct a manner as possible.

The comet which forms the subject of the investigations for which the Medal has been awarded was discovered by M. Faye at the Observatory of Paris on November 22, 1843, about five weeks after its perihelion passage. Notwithstanding this, the impossibility of representing its motion by parabolic elements was soon evident, and the limited extent of the orbit being indicated a few weeks after discovery, the attention of observers was specially directed to the comet, and observations were made as late as April 10, 1844, by M. O. Struve with the Pulkowa refractor. A pretty complete determination of the orbit from the observations of 1843–44 was made by Le Verrier, who also calculated the perturbations during the ensuing revolution due to the action of *Venus*, the Earth, *Mars*, *Jupiter*, and *Saturn*, and thus obtained the elements corresponding to the next perihelion passage on April 3, 1851. At that time Professor Challis was giving close attention to the observation of comets by means of the Northumberland Equatoreal at Cambridge, and with the aid of the Ephemeris founded upon Le Verrier's elements he succeeded in observing Faye's Comet as early as November 28, 1850, continuing his observations until March 4, or nearly a month after it was seen elsewhere. Observations at this second appearance were confined to the Cambridge Observatory, and the Observatories at Pulkowa, and Harvard College, U.S. Le Verrier issued an Ephemeris corrected by the earlier observations at Cambridge, but did not further continue his calculations.

It does not appear that any computations bearing upon the return of the comet in 1858 were made, beyond an approximate Ephemeris circulated from the *Nautical Almanac* office. With the aid of this Ephemeris the comet was detected by Dr. Bruhns with the Berlin Refractor on September 7, and observations were continued till October 16.

Early in 1859 the Society of Natural Sciences at Dantzic

offered a prize for the determination of the orbit of Faye's Comet from the three appearances of 1843–44, 1850–51, and 1858, the memoirs to be sent in at the beginning of January, 1861. I am not aware if it was this circumstance which directed Professor Möller's attention to the comet, but it was at this period that his investigation of its motion was commenced. His first paper appeared in No. 1259 of the *Astronomische Nachrichten*, and is dated from Lund in April 1860. Adopting the elements which Le Verrier had brought forward to 1851, he compares them with the series of Cambridge observations, commencing on December 25, 1850, and forming normal places, he obtains corrected elements for 1843. From this amended orbit he calculates the perturbations of *Venus*, the Earth, *Mars*, *Jupiter*, *Saturn*, and *Uranus*, for the intervals over which the comet was observed in 1843–44 and 1850–51, and then makes a comparison with the observations at both appearances. Normal places are again formed and the orbit is further corrected. Thus pretty accurate elements are derived for 1843 November 9, and 1851 February 20, which form the definite results of Professor Möller's first communication.

In No. 1295 of the same periodical he brings the observations made in 1858 to bear upon the orbit, having first calculated the perturbations during the comet's visibility as before, and introduces Encke's hypothesis of a resisting medium, the existence of which had been rendered so probable from the researches made by that astronomer on the motion of the comet which bears his name. He considered that the calculations detailed in this second memoir (involving, it will be seen, three appearances of the comet) proved that an hypothesis was needed to explain the motion of Faye's Comet, and that one of the character advocated by Encke would be nearly correct. This conclusion naturally attracted the attention of astronomers, because Faye's Comet at no time approaches the Sun within the Earth's mean distance, and the effect of a resisting medium could hardly have been expected to have been appreciable upon the comet's motion. This question is further examined by Professor Möller in No. 1314 of the *Astronomische Nachrichten*, with reference to a communication made by Baron Plana to the Turin Academy, and again in No. 1317, with respect to the hypothesis of Valz on the density of the ether.

In No. 1358, Professor Möller has some further researches on the orbit of the comet from the observations at the first three returns, in which additional refinement is introduced in the computation of the planetary perturbations, and a close representation of the observations is produced, the orbit for 1858 being considered not only exact enough for prediction at the ensuing return to perihelion, but for connecting with accuracy the observations that might be then made, with the earlier ones. In his next communication to the *Astronomische Nachrichten*, with reference to Faye's Comet (No. 1522), he made an announcement

which is of especial interest, as according with the preconceived ideas of astronomers on the improbability of resistance to the motion of the comet from the etherial medium: he there states distinctly that he had given up the idea of action from this cause, and had satisfied himself that the observations at the first three appearances were perfectly accordant, without any hypothesis whatever. This conclusion, it must be observed, had not been arrived at without the introduction of one refinement after another, and wading through a very heavy mass of computations of a difficult and laborious character. In the same memoir, slight corrections are again applied to the elements in 1843–44, 1850–51, and 1858, and the orbit of the latter year is carried forward to the ensuing return in the autumn of 1865, by the calculation of the planetary perturbations in the interval; Professor Möller also supplied an accurate Ephemeris from July 1 to November 22. This memoir, in which precise prediction of Faye's Comet was first made, is dated from Lund, in March 1865. The comet was detected by D'Arrest, with the Copenhagen Refractor, on August 22, and, in publishing his first observation, he wrote, "The agreement with Professor Möller's calculations, after the seven years' invisibility of the comet, is extraordinary, in fact almost complete," the difference being only $1^s{\cdot}7$ in Right Ascension and $4''$ in Declination: notwithstanding the refinement introduced into Professor Möller's work, probably no one had anticipated so admirable a confirmation of the skill and labour with which it had been conducted. In the *Astronomische Nachrichten*, No. 1548, Professor Möller continued his accurate Ephemeris to April 14, 1866.

Resuming his investigations in 1870, he found that when the observations of 1865–66 were brought to bear upon them, in addition to those at the three previous returns, the agreement between calculation and observation was not so satisfactory as before. He accordingly did not shrink from a complete revision of the computation of perturbations, aiming at the utmost attainable accuracy: the normal positions were thoroughly examined, improved places of some of the comparison stars introduced, and, in treating the equations of condition, a term was included for the correction of the mass of *Jupiter* deduced by Bessel from the elongations of his satellites, which had been adopted through the whole of Professor Möller's work. Thus the most precise orbit it was possible to determine for 1865, as the ground-work for further computation, was obtained. The value of *Jupiter's* mass indicated by these laborious researches on the motion of Faye's Comet was found to be $\frac{1}{1047{\cdot}788}$, that assigned by Bessel being $\frac{1}{1047{\cdot}879}$, these values agreeing within their probable errors. Details of these additional computations will be found in *V. J. S.*, vol. vii.

The perturbations during the ensuing revolution were calcu-

lated with all strictness, and elements assigned for the epoch of the comet's return to perihelion in 1873. Its track in the heavens in that year was unfavourable for observation, and only four positions were secured between September 3 and December 23. By the Marseilles observation on the former date, Professor Möller's predicted place was less than six seconds of arc from the true one, a really marvellous confirmation of the accuracy with which his investigations of the comet's motion during forty years had been conducted.

Once more he determined the effect of perturbation during a whole revolution, with the same refinement as previously, and in September 1878 communicated to the Academy of Sciences at Stockholm elements and an accurate Ephemeris for the actual appearance of 1880–81, and, as we have just seen, with renewed success, though the effect of planetary attraction in this interval has been greater than in any other revolution since the comet was discovered.

Not content with following the comet through each successive revolution from 1843, Professor Möller, in a paper presented to the Academy of Stockholm in January 1873, computes the perturbations backward to December 25, 1838, and ascertains the effect of a pretty near approach to *Jupiter* in March 1841, when the distance of the comet from the planet was less than two-thirds of the Earth's mean distance from the Sun: thus the perturbations have been rigorously calculated by Professor Möller from December 25, 1838, to January 13, 1881. In the same paper he also examines the circumstances of a much nearer approach near the passage of the ascending-node in 1816, and infers a minimum distance of about 0·3.

It is necessary to bear in mind that the class of computation involved in Professor Axel Möller's investigations is, so to say, of a high order, and does not admit, as is often the case with heavy masses of astronomical calculations, of being set out for the ordinary computer. Considerable mathematical knowledge, and knowledge of refined methods, and of their adaptability to particular cases is absolutely essential; and Professor Möller's success has proved that he possesses this knowledge in an eminent degree. I believe that you will consider that your Council has been fully justified in awarding to Professor Möller the highest mark of approval and respect which the Royal Astronomical Society has it in its power to bestow, and I further entertain the belief that this award will give satisfaction to astronomers in all countries, as encouraging a very monotonous and tedious, but not less important, branch of astronomical research.

The President then, delivering the Medal to the Foreign Secretary, the Earl of Crawford and Balcarres, addressed him in the following terms:—

In transmitting our Gold Medal to Professor Axel Möller, I ask you, my Lord Crawford, to assure him of the great interest

which the Royal Astronomical Society has taken in his researches on the motion of Faye's Comet during the long period over which they have extended, and to convey to him the best wishes of the Society that health and energy may be spared him to continue these researches, which have been attended with such admirable and well-merited success.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected :—

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J. R. Hind, Esq., F.R.S., Superintendent of the *Nautical Almanac.*

Vice-Presidents.

Arthur Cayley, Esq., M.A., LL.D., D.C.L., F.R.S., Sadlerian Professor of Pure Mathematics, Cambridge.
Edwin Dunkin, Esq., F.R.S.
William Huggins, Esq., LL.D., D.C.L., F.R.S.
E. J. Stone, Esq., M.A., F.R.S., Radcliffe Observer.

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H. J. S. Smith, Esq., M.A., LL.D., F.R.S., Savilian Professor of Geometry, Oxford.

ERRATUM.

In the *Monthly Notices*, vol. xli. p. 142—Last Contact with De La Rue reflector—*for* $3^h\,38^m\,2^s{\cdot}0$ *read* $3^h\,28^m\,2^s{\cdot}0$.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLI. MARCH 11, 1881. No. 5.

J. R. HIND, Esq., F.R.S., President, in the Chair.

Squire Thornton Stratford Lecky, Esq., 1, Morningside Road, Bootle, Liverpool;

James Henry Mitchiner, Esq., Burgos House, Sydenham Road North, Croydon; and

The Rev. T. R. Terry, M.A., Magdalen College, Oxford;

were balloted for and duly elected Fellows of the Society.

On the Determination of the Value of the Parallactic Inequality in the Motion of the Moon. By James Campbell, Esq., and Edmund Neison, Esq.

In the *Monthly Notices* for November 1880, there is a paper by Mr. Stone, F.R.S., criticising a paper of ours which appeared in the *Monthly Notices* for May and June 1880, and is entitled, "On the Determination of the Solar Parallax by means of the Parallactic Inequality in the Motion of the Moon." This paper by Mr. Stone calls for a reply from us.

Mr. Stone's paper may be divided into three portions, under the following heads—

(1) An account of the method adopted by him in his determination of the value of the parallactic inequality published in 1867.

(2) A demurrer to the manner in which the effect of the variation in the irradiation at the lunar limb is deduced in our investigation.

(3) A criticism of the hypothesis of the possible existence of an equality of long period in the apparent value of the coefficient of the parallactic inequality.

These three portions we propose to take in order.

(1) Mr. Stone states that we have formed an incorrect idea of the method employed by him in the investigation of which the only published account is the short abstract in the *Monthly Notices* for May 1867, an account which, though professing to describe the method which was followed, yet makes no reference to the additional precautions and corrections which it seems were actually employed by Mr. Stone. Through interpreting in the most natural manner the account given in this abstract, it seems that its imperfect nature has led us to form an incorrect idea of the method employed. This we must regret. Yet we submit that the carefully guarded manner in which we describe, on pp. 407–409 of our paper, the conclusions which we draw as to the probable method followed by Mr. Stone, and the clear manner in which we restrict our criticisms to Mr. Stone's value as thus hypothetically interpreted, must be regarded as effectually vindicating us from the unnecessary strictures in this paper by Mr. Stone.

We accept without demur Mr. Stone's statement that the specific corrections hypothetically applied by us to his result are really inapplicable. This naturally follows from the fact that Mr. Stone's investigation had a different basis to what we had been led to imagine. We learn now from Mr. Stone what is the real basis of his investigation, and consequently can form, now, a clear idea of the value of the result. The only conclusion which it seems can be drawn is that the value obtained by Mr. Stone must require the application of a considerable number of large corrections, and even then can only be regarded as of most imperfect nature.

Thus, from Mr. Stone's account, it seems, that actually passing over the great mass of indirect comparisons between observation and Hansen's tables, he employed the far inferior series of comparisons between observation and Burckhardt's tables, merely applying some small corrections to render these latter tables somewhat less faulty. Further, it seems that in round numbers more than three-fourths of the observations employed by Mr. Stone were compared with Burckhardt's tables, and only one-fourth with Hansen's tables. Moreover, it would appear that some two-thirds of the observations made use of by Mr. Stone were made with the Greenwich altazimuth instrument, and only about one-third with the transit circle.

Now, the errors in Burckhardt's tables are so large and so numerous that, to render the tables fit for use in deducing corrections to any of the lunar inequalities, it is absolutely essential that a considerable number of corrections should be applied. Moreover, in most cases these corrections must be applied to the individual observations, and cannot be adequately applied to the mean results. This is a point which we strongly urge, and is founded on the considerable experience which one of us has had in the comparison of these tables with observations. The errors

introduced by the employment of these tables are rectified in a very small degree by merely applying a correction to the mean results for "errors in the coefficient of Burckhardt's variation, annual equation, and term 3D." (Mr. Stone does not tell us what were the separate corrections he deemed these coefficients to require.)

Besides these errors in the values of the coefficients of the terms actually employed by Burckhardt in his Lunar Tables, these tables are vitiated by the imperfections due to the omission of numerous terms found by subsequent theorists to have sensible coefficients. These omissions will introduce systematic errors into any results based on his tables. It may be urged that in the mean of nine or thirteen years' observation these errors may mutually destroy each other. They *may*: but this is a point which must be proved, not assumed.

But the greatest objection to the results obtained by Mr. Stone lies in the fact that they practically are founded on the altazimuth observations, and mainly on the altazimuth observations compared with Burckhardt's tables.

The altazimuth observations are liable to so many systematic errors that they are entirely unfitted for use in determining the value of the parallactic inequality, especially when they are compared with faulty tables. The observations for the positive and negative values of the parallactic inequality are systematically made with this instrument under entirely different conditions, and are affected, therefore, by every systematic error inherent to such a condition. The observations at the two quarters are not only made at different times of the night, one long before and the other usually long after midnight, but they are made on opposite sides of the meridian, and in entirely different portions of the heavens. All these must tend to introduce grave systematic errors. Further, from the conditions under which the observations are made, not only will there be grave systematic errors introduced through the variations in the semi-diameter of the Moon, but there will be also serious systematic errors introduced through the corrections both for parallax and refraction. The difficulties in dealing with the variations in the apparent diameter of the Moon seem enormous. The diameter of the Moon derived from observations in altitude is known to be entirely discordant with that derived from observations in azimuth, differing, in fact, by over one second; and this, whilst conclusively establishing the existence of serious systematic errors in the observations, renders it quite inadmissible to apply a constant correction for semi-diameter, as is done by Mr. Stone. Which is the true correction to be employed, that furnished by the observations in altitude, or that furnished by the observations in azimuth? The resulting values of the parallactic inequality will differ by over half a second. And if neither, but some intermediate value, how can it be determined what intermediate value is correct? Moreover, though the personalities in observing diameters with the transit circle have

been shown to be small, those in observations with the altazimuth are known to be very large.

For this reason we consider that the altazimuth observations are unfitted for use for the purpose of determining the value of the parallactic inequality in the motion of the Moon, unless care be taken to eliminate all these sources of systematic error by the application of adequate corrections to the separate observations; and we believe that in this opinion we shall be supported by all those astronomers who have had experience in the comparison of these altazimuth observations with an adequate lunar theory.

As, from Mr. Stone's account of his investigation, he only applied to mean results assumed corrections for errors in the variation, annual equation, and term 3θ (Mr. Stone's 3D), together with a constant correction for semi-diameter, it would appear that the corrections he has applied are altogether inadequate to properly eliminate the various sources of systematic error affecting the altazimuth observations.

(2) The second point raised by Mr. Stone is a very simple one. In our paper we determined the coefficient of the irradiation constant from the equations—

$$\delta P + 0{\cdot}75\,\delta I = \text{apparent value of parallactic inequality from summer observations.}$$

$$\delta P + 0{\cdot}25\,\delta I = \text{apparent value of parallactic inequality from winter observations.}$$

Mr. Stone objects that the values taken for the coefficients of δI are assumptions, and that their correctness must be proved. He shows that by varying the ratio between these two coefficients you make important changes in the value of the true parallactic inequality. This objection would be perfectly valid were the ratio between these two coefficients a mere assumption, as imagined by Mr. Stone. But this is not so. On p. 445 of our paper it is clearly stated that these coefficients, and therefore the ratio between them, were derived from the observations themselves. This ratio of three to one arises practically from the fact that three times as many observations are made on a bright sky in the summer as are made in the winter. The actual figures given by the observations range usually within a small distance on either side of the mean values 0·25 and 0·75.

It has been already stated that the coefficient i of the constant δI was assumed to vary directly as the brightness of the sky at the epoch of the transit of the Moon. This leads to results the same as employed by Prof. Newcomb. But even if a different law be assumed, it will be found that the ratio between the two coefficients is not materially affected. Originally, a somewhat different law was assumed by one of us in these investigations, and this result was found to hold; but it was abandoned because, on comparison with the results furnished by the observa-

tions, it was found that the variation in the irradiation increased at a greater rate than had been assumed.

There is, however, some additional information on this point which may now be given; which, indeed, would have been included in our former paper had it not been deemed inadvisable to increase its already great length. This is a proof of the accuracy of the law we have assumed for the variation in the irradiation at the limb; the law being that the coefficient i varies directly as the brightness of the sky, or that the irradiation at the limb varies directly as the contrast in brightness between the limb of the Moon and the sky it is seen against, or inversely as the brightness of the sky.

In the reduction of the Greenwich lunar observations which has been undertaken by one of us, every observation is supposed to require the small correction $i\delta I$, where i varies from 0 to 1·5 according to the law stated. Let the observations be divided into four groups; those for which the value of i lies between the limits—

$i = 1{\cdot}5$ and	$i = 1{\cdot}2$	mean $i = 1{\cdot}35$
$i = 1{\cdot}2$	$i = 0{\cdot}9$	$i = 1\,05$
$i = 0{\cdot}9$	$i = 0{\cdot}3$	$i = 0{\cdot}60$
$i = 0{\cdot}3$	$i = 0{\cdot}0$	$i = 0{\cdot}15$

From each of these groups let δI be determined, and if each group yields the same value, then the assumed law must accurately represent the true law; but if they be discordant, then the coefficient i must be made to vary according to some different law which will make all these values alike. It is not difficult to eliminate from the observations for any group of years nearly all the other corrections, leaving merely δI. The Greenwich observations for the nine years 1868–1876 were thus reduced, leaving out the last group, as the coefficient of i is too small to give trustworthy results. The number of observations included in the first group were but small, so that its weight was small. Of course, the real importance lies in the results obtained from the two middle groups, for if these give identical values of δI, then the assumed law is proved. The results are—

For mean value of $i = 1{\cdot}35$	$\delta I = -1''{\cdot}30$	Weight 1
$= 1{\cdot}05$	$= -1{\cdot}25$	5
$= 0\,65$	$= -1{\cdot}32$	7

This completely confirms the sufficiency of the assumed law. It may be added that these results are based on the complete series of meridian results for the period specified, or on nearly one thousand observations.

Mr. Stone has also criticised the value we find for δI. He states that even after applying the constant corrections given by us, the separate values are very discordant. Apparently

Mr. Stone has overlooked the following paragraph in our paper (pp. 464, 465)—

"When the values assigned to the corrections δV, δM, δK, δl, come to be substituted in the expressions for δI during the different years, it will be found that there still remain considerable outstanding errors. These are partly due to the fact that constant values have been assigned to these corrections, whereas the observations show that they are subject to some variation, due to the fact that Hansen's tables require one or two more corrections not included in the preceding expression. Strictly speaking, in each year should have been used the values found for δV, δM, δK, δl, from the observations of that year, as in that manner these neglected corrections would have been practically eliminated. The method actually adopted for determining the value of P gives results which are sensibly as accurate as if the stricter method had been employed, for the corrections are multiplied by such small coefficients that slight variations may be neglected. When the expressions for δI are considered, the use of the stricter method markedly reduces the values of the outstanding errors in the years 1876, 1869, 1868." [Almost the only large outstanding errors.] "But it must be remembered that the apparent errors of these years are much exaggerated by their being derived from equations in which δI is multiplied by a small coefficient, so that in dividing by this coefficient the apparent errors are increased to double, triple, or even quadruple their true amount. In the value for the irradiation which has been derived from the observations and actually employed, all the equations are grouped together, and appear with their proper weight according to the largeness of their coefficients of δI. The result, therefore, is entitled to far more weight than may appear from a consideration of the outstanding discordances, so many of which have been thus exaggerated."

It may be added that the results were computed on both systems, and found to agree. Since then the principal cause of the variation in the apparent value of δM and δK (for the latter, Prof. Newcomb's value was used in our paper) has been found to arise from the need of a correction to the term with the argument α or the mean anomaly of the Moon. This was surmised in our paper.

Therefore, as it has been shown that the law of the variation in the irradiation at the limb, the actual amount of this variation, and the ratio between the coefficients with which it appears in the final equations are all derived directly from the observations themselves, it must be held that the correction for the effect of this variation in irradiation is also furnished directly by the observations, and is altogether free from that arbitrary character imagined by Mr. Stone.

We hold, therefore, that we have vindicated this portion of our investigation from the demurrer raised against it by Mr. Stone.

(3) In his criticisms on the portion of our paper dealing with the hypothetical forty-five-year inequality in the apparent value of the parallactic inequality, Mr. Stone seems to have entirely misunderstood our position, and it might be imagined that we supposed our paper to contain a proof of the existence of this new term. But that is a complete misconception, as throughout we refer to this inequality as purely hypothetical, and one whose existence has yet to be proved. (See for instance page 467 lines 16–28, and page 468 lines 6–8.) As we so clearly state in our paper, we do not regard the existence of this term as established, for reasons we have already given, but we do consider that there is strong evidence in favour of the possible existence of such an inequality.

Let us recapitulate this evidence.

Sir G. B. Airy had reduced on a uniform system, and by reference to uniform lunar tables, the entire series of Greenwich lunar observations made between 1750 and 1851; and from these observations there have been deduced twenty-one values of the coefficient of the apparent parallactic inequality, these values being strictly comparable *inter se*, and each being based on some six or seven hundred observations, spread over a period of nine years. When these values come to be compared, it is found that they exhibit grave discordances amounting to over a second of arc in many cases, but that these discordances are obviously periodical in form. If these discordances be assumed to be due to the existence of a hitherto unsuspected term in the motion of the Moon, giving rise to an apparent inequality of forty-five years' period in the values found for the parallactic inequality, all these discordances disappear from twenty of these nine-year groups, and the only discordance is for the small group of years 1847–1851. There can be no doubt about this constituting strong evidence in favour of the existence of such an inequality. It is not the case, as imagined by Mr. Stone, that the only evidence in its favour lies in the early observations made prior to 1816; for a reference to the table on page 403 of our paper shows serious discordances in the observations between 1816 and 1846 which are completely removed when the inequality is introduced. These as strongly support its existence as any of the earlier groups. We have, therefore, the observation for the ninety-seven years 1750–1846 in its favour, and only those of the five years 1847–1851 against it. Mr. Stone urges that the observations of the period 1847–1851 are superior to those of the preceding century. Undoubtedly, but they are not sensibly superior to those made between 1835–1846, or much better than those between 1816–1835.

Further, it is found that when the observations made between 1862–1876 are reduced and compared with Hansen's tables as corrected, they give a value of the apparent parallactic inequality which is in perfect accord with the assumed existence of this forty-five-year inequality. It is here, however, that the weak

point appears, and is duly noted by us; these observations are not only made with a different instrument, but they are not referred to the same tables as the observations employed by Sir G. B. Airy, and the two results are not *strictly comparable*. It is true that the fundamental differences between the two tables (Airy and Hansen *both corrected from observation*) are not so large as to render them likely to be of much importance, and the two instruments were of analogous nature, but these differences do decrease the weight to be attached to the close accord found to exist between the results.

Mr. Stone, however, to show that this hypothetical inequality has no real existence, brings forward the values which he has deduced for the apparent value of the parallactic inequality, by comparing the altazimuth observations of the Moon for 1848–1861 with Burckhardt's lunar tables, and for the years 1862–1878 with Hansen's tables. But as these observations are not only made with an entirely different description of instrument, they are neither comparable with the results obtained by Sir G. Airy or by ourselves from the meridian observations, nor, as they are referred to entirely different tables, are they even comparable with each other. For, be it remembered, these values which are being discussed are not the values of the coefficient of the true parallactic inequality, but of the apparent inequality which is affected by the variations in the irradiation at the limb, and may be therefore entirely different in different instruments. If the results quoted by Mr. Stone were comparable, then they would be utterly discordant with the hypothesis of a forty-five-year inequality. But they most certainly are not comparable. The results deduced through the exceedingly faulty tables of Burckhardt, and affected by all the consequent systematic errors, cannot be compared with the results deduced through the more exact tables of Hansen without the application of a far more extensive series of corrections than has been applied by Mr. Stone;* and the results deduced from the altazimuth observa-

* With the view of showing the justness of this statement that the corrections applied by Mr. Stone to the results from Burckhardt's tables are entirely insufficient to eliminate the errors introduced by the imperfection in the theoretical basis of the tables, we have computed the true corrections to be applied to the results for the three years 1854–56, taking into account the errors in some twenty-two terms, instead of only three. The results are:—

	Stone. Correction for error in 3 terms.	Neison and Campbell. Correction for error in 3 terms.	Correction for remaining 18 terms.	
1854	= +1″·56	+1″·47	= −1″·85	Sum = −0″·38
1855	= +1·56	+1·62	= −1·96	= −0·34
1856	= +1·56	+ ·98	= +0·97	= +1·95

In these particular years the errors are not near these maximum values, and it may be stated that the constant correction used by Mr. Stone of +1″·56 is a variable quantity which may take all values from about +3″·5 and −1″·5.

tion cannot be compared with the results of the meridian observations, as they are affected by constant errors due to the variation in the irradiation at the limb, which must differ in the two different classes of instruments, even had not the results derived from the altazimuth observations been affected by serious systematic errors.

Mr. Stone, however, has appealed to the observations of the period 1850–1860 as being decisive against this forty-five-year inequality. Are they really so? This is a matter admitting of proof, for there exists a considerable number of observations made with the transit circle and indirectly compared with Hansen's tables. They form, in fact, the very data which we had imagined Mr. Stone to have employed in his investigation, being so much superior to those actually employed by him. These observations are given in the volume of Greenwich Observations for 1859, and from them we select all the observations made with the transit circle, and comprised within the years 1851–1858. They furnish us with results strictly comparable with those for the years 1862–1876 made use of by us in our previous paper.

The results are as follows:

1851	43 observations	$\delta(P) = -2''03$
1852	31	$= -3\cdot17$
1853	42	$= -1\cdot50$
1854	45	$= -2\cdot45$
1855	36	$= -1\cdot80$
1856	37	$= -2\cdot49$
1857	38	$= -2\cdot46$
1858	53	$= -2\,65$
	325 observations	$\delta(P) = -2\cdot32$

To reduce this result to what would have been obtained had they been compared directly with Hansen's tables, it is necessary to apply the constant correction for the difference between the two tables at midnight and at the epoch of observation. This amounts to (see page 401 of our paper)—

$$\tfrac{1}{8}\Delta V + \tfrac{1}{12}\Delta Q = -0''52.$$

The result is then the value which would have been obtained had the observations been compared directly with Hansen's tables. It is

$$(P) = -\{126''46 - 2''32 - 0''52\} = -123''62.$$

This value has now to be corrected for the errors in Hansen's

tables. These have been approximately calculated, and the mean correction for the eight years is

$$+0\cdot10\,\delta V - 0\cdot13\,\delta M + 0\cdot07\,\delta A = +0''\cdot14.$$

The correction for the term of the form $\delta K \sin k$, which gives the correction required by Hansen's tables for errors in the equation of the centre and the Jovian evection, was calculated, but in the mean of the eight years nearly vanishes; it is—

$$+\kappa\,\delta K \quad = -0''\cdot03.$$

For the correction required by the semi-diameter we have taken that given in the Greenwich Observations for 1863, namely

$$+s\,\delta S \quad = -0''\cdot18.$$

This is somewhat greater than the value found by one of us for the period 1851–1858. The sum of these corrections amounts to

$$+0''\cdot14 - 0''\cdot03 - 0''\cdot18 \qquad = -0''\cdot07.$$

Therefore, the value of the apparent parallactic inequality will be

$$(P) = -\{123''\cdot62 - 0''\cdot07\} = -123''\cdot55.$$

This is the result furnished by the 325 meridian observations made during the eight years 1851–1858.

It is easy now to compare this result with those obtained in our previous paper. We have added the correction required for the forty-five-year inequality—

1851–1858	$(P) = -123''\cdot55 - \frac{4}{7}B$
1862–1869	$= -125\cdot09 + \frac{2}{9}B$
1870–1876	$= -125\cdot13 + \frac{1}{8}B.$

Giving to B its value, $B = +1''\cdot20$, these become

1851–1858	$(P) = -124''\cdot23$
1862–1869	$= -124\cdot09$
1870–1876	$= -124\cdot13.$

To these may be added Airy's value

1751–1846	$(P) = -124''\cdot13.$

It will be evident, therefore, that the entire series of meridian

observations unite in supporting this hypothesis of a forty-five-year inequality. More than that we do not wish to say at present, unless it is to add that if this inequality really exists it must be possible to find out its origin and calculate from theory its value.

In reducing these observations, care was taken to determine the value they furnish for the irradiation constant δI so as to enable the value of the true coefficient of this parallactic inequality to be obtained. They gave the value

$$0{\cdot}50\,\delta I = -1''{\cdot}73 + \kappa\delta K + t\delta T.$$

The value of the correction depending on the term $\delta K \sin k$ was calculated, and the mean for the eight years was

$$\kappa\delta K = +0''{\cdot}46.$$

The term depending on the factor $t\,\delta T$ or on the systematic errors in the Greenwich clock star places, was calculated to be approximately

$$t\delta T = +0''{\cdot}72.$$

Hence

$$0{\cdot}50\,\delta I = -1''{\cdot}73 + 1''{\cdot}18 = -0''{\cdot}55.$$

Therefore

1851–1858	$\delta I = -1''{\cdot}10$
1862–1869	$= -1{\cdot}06$
1870–1876	$= -1{\cdot}18.$

The weight of the first of these is considerably smaller than that of the last two groups.

Employing these values of the irradiation correction, we have

1851–1858	$\delta P = (P) + 0{\cdot}44\,\delta I = -124''{\cdot}72$
1862–1869	$= (P) + 0{\cdot}46 \quad = -124{\cdot}62$
1870–1876	$= (P) + 0{\cdot}50 \quad = -124{\cdot}66.$

The accordance between the different results is complete, and leaves it necessary for us to add nothing.

(4) In concluding this paper it may be as well perhaps to state that further information on the true value of the parallactic inequality and on the rate and amount of the variation in the irradiation at the limb, but based on the entire available Greenwich Observations, will be furnished in all detail in a memoir on the corrections required by Hansen's lunar tables which is now

approaching completion in the hands of one of us. This memoir will contain an investigation of the systematic errors of the altazimuth observations.

Arkley Observatory,
Barnet, Herts:
1881, *March* 10.

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*Effect on the Moon's Movement in Latitude, produced by the slow Change of Position of the Plane of the Ecliptic.** By Sir G. B. Airy, K.C.B., Astronomer Royal.

It appears that, in consequence of the action of the Planets, the position of the plane of the orbit round the Sun, described by the centre of gravity of the luno-terrestrial system, undergoes a continual change; which, as estimated for a great number of years, may without sensible error be considered as a uniformly progressive change. The plane of the orbit in the year 1900+T years crosses the plane of the orbit in the year 1900 in a line whose longitude (as estimated from the zero of longitude customary in astronomy) is 172° 50′ nearly; and the plane of the orbit is inclined annually by 0″·479, in such a direction that the portion of the plane which is in longitude 262° 50′ is carried towards the north pole of the ecliptic of the year 1900. (Le Verrier, *Annales*, tome ii. p. 105.)

There is no reason to suppose that this small change in the position of the Sun's apparent orbit will produce any discoverable effects on the ecliptic-longitude or radius vector of the Moon; but we may properly examine what is its effect on the Moon's latitude, as referred either to the ecliptic-plane of 1900 or to that of 1900+T years.†

Now, referring all forces to the plane of the ecliptic in the year 1900, and putting z for the linear elevation of the Sun at any time above that plane; v being the geocentric longitude of the Moon, and V the longitude of the Sun as viewed from the luno-terrestrial centre of gravity, and other letters having their usual acceptation: it is easily found that—

* On the general question here considered, see *Mécanique Céleste*, Book vii., Chap. i. Also Cayley, "A third Memoir on the Problem of Disturbed Elliptic Motion" (*Mem. R.A.S.* vol. xxxi. [1863], pp. 43–56), and Hansen's *Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen* (Leipzig, 1862). Laplace's conclusion is that the Motion in regard to the variable ecliptic is the same as it would have been in regard to a fixed ecliptic. Hansen's correction of this result is cited in the footnote further on, p. 271.—Ed.

† It appears (p. 491 of the *Darlegung*) that the change produces also a long inequality in the Moon's longitude, the ratio of the coefficients being nearly 8 sin ½J : 1, or numerically about 0·36; but the amount of the irregularity in latitude is not sensibly affected by this circumstance.—Ed.

Force in z depending on the Sun's elevation above the ecliptic of 1900

$$= +3\frac{\sigma}{A^3} \cdot \left(\frac{A}{R}\right)^3 \cdot \frac{r}{a} \cdot \cos 1 \cdot \cos \overline{|v-V|} \cdot \frac{z_\sigma}{R}.$$

The value of $\frac{\sigma}{A^3}$, using the same constants which I have used in a preceding paper, is ·0055953, and $+3\frac{\sigma}{A^3} = +·016786$.

For the other factors which contain inequalities of the movements of the Earth and Moon, I propose to neglect all those periodic terms which are not strictly and necessarily connected with the present problem, or those whose periodic times are not simply related to those of the principal terms. The only inequalities which are so connected are those depending on $\overline{|v-V|}$ or on $\overline{|nt-Nt|}$, where

$$\frac{N}{n} = \frac{\text{Sun's mean angular motion}}{\text{Moon's mean angular motion}} = ·0748013.$$

For the terms which contain other inequalities, I use only mean values of the coefficients.

Thus we have

$$\left(\frac{A}{R}\right)^3 = 1·0004215.$$

$$\frac{r}{a} \cdot \cos 1 = ·9995477 - ·0075470 \cdot \cos \overline{|2nt-2Nt|}.$$

$$\left(\frac{A}{R}\right)^3 \frac{r}{a} \cdot \cos 1 = ·9999692 - ·0075501 \cdot \cos \overline{|2nt-2Nt|}.$$

For the two remaining terms, I shall express their value not singly, but in combination. Now, $\frac{z_\sigma}{R}$ is the value (referred to radius) of the Sun's apparent angular elevation above the ecliptic of 1900, or is

$$= +·479 \times \sin 1'' \times \text{number of years after 1900} \times \sin \overline{|V-C|},$$

$$= +·479 \times \sin 1'' \times \frac{t}{84} \times \sin \overline{|V-C|}, \text{ (84 being the number of units of time in a Julian year),}$$

where $C = 172° 50'$.

I shall use the letter B for $+·479 \times \sin 1'' \times \frac{1}{84}$. Then the Sun's angular elevation above the ecliptic $= Bt \cdot \sin \overline{|V-C|}$; and the product of the two last terms in the expression for "Force in z" is

$$\frac{Bt}{2} \times \{+\sin \overline{|v-C|} - \sin \overline{|v-2V+C|}\}.$$

Now, $v = nt + ·01148 \cdot \sin \overline{|2nt-2Nt|}$ (including only those

small terms of which we have spoken), and V has no term depending on $|nt-Nt|$; and the product of the two terms therefore is

$$+\frac{Bt}{2}\times\left\{\begin{array}{l}+\sin|nt-C|-\sin|nt-2Nt+C|\\+(\cos|nt-C|-\cos|nt-2Nt+C|)\times\cdot01148\,.\,\sin|2nt-2Nt|\end{array}\right\}$$

$$=\frac{Bt}{2}\times\left\{\begin{array}{l}+\sin|nt-C|-\sin|nt-2Nt+C|\\+\cdot00574\,.\,\sin|3nt-2Nt-C|+\cdot00574\,.\,\sin|nt-2Nt+C|\\-\cdot00574\,.\,\sin|3nt-4Nt+C|-\cdot00574\,.\,\sin|nt-C|\end{array}\right\}$$

Multiplying this by

$$+\cdot016786\times\{+\cdot99997-\cdot00755\,.\,\cos|2nt-2Nt|\}$$

(found above for $3\frac{\sigma}{A^3}\times\left(\frac{A}{R}\right)^3\cdot\frac{r}{a}\cos l$), we have for the force in z,

$$+\cdot0083930\,.\,Bt\times\left\{\begin{array}{l}+\cdot99997\,.\,\sin|nt-C|-\cdot99997\,.\,\sin|nt-2Nt+C|\\+\cdot00197\,.\,\sin|3nt-2Nt-C|+\cdot00951\,.\,\sin|nt-2Nt+C|\\-\cdot00197\,.\,\sin|3nt-4Nt+C|-\cdot00951\,.\,\sin|nt-C|\end{array}\right\}$$

$$=Bt\times\left\{\begin{array}{l}+\cdot0083130\,.\,\sin|nt-C|-\cdot0083130\,.\,\sin|nt-2Nt+C|\\+\cdot0000166\,.\,\sin|3nt-2Nt-C|-\cdot0000166\,.\,\sin|3nt-4Nt+C|\end{array}\right\}.$$

This is the expression for the force which is to be used on the first side of the Equation of the Factorial Table.*

The equation is the following—

$$\text{Force}=\left\{\begin{array}{l}+\delta l\times(+1\cdot007945+\cdot0428\,.\,\cos|2nt-2Nt|)\\+\frac{d\delta l}{dt}\times(\quad\cdot000000+\cdot0278\,.\,\sin|2nt-2Nt|)\\+\frac{d^2\delta l}{dt^2}\times(+\cdot999545-\cdot0075\,.\,\cos|2nt-2Nt|)\end{array}\right\}.$$

A very little trial and consideration will show that the form to be assumed for δl, whose substitution in the last expression will produce such a "force" as was found above, must be the following—

* The Factorial Table to which reference is made here, is a Table embodying three equations; which are prepared by forming the three equations of motion, each of which is expressed in terms of r, v, and z; and differentiating these equations (including both the geometrical ordinates, and the terrestrial and solar mechanical forces due to these geometrical ordinates) in such a manner as to exhibit the magnitudes of new forces which must be introduced in order to produce ordinates $r+\delta r$, $v+\delta v$, $z+\delta z$. I hope shortly to publish in detail, in a "Numerical Lunar Theory," the steps of formation of these equations. In the present instance, only one of the equations of motion is employed, and it is brought to its simplest state by neglecting all variable quantities not necessarily involved in the problem.

$$\delta l = \left\{ \begin{array}{l} +Pt \,.\, \sin \left| \overline{nt-C} \right| + Qt \,.\, \sin \left| \overline{nt-2Nt+C} \right| + Rt \,.\, \sin \left| \overline{3nt-2Nt-C} \right| + St \,.\, \sin \left| \overline{3nt-4Nt+C} \right| \\ +\ p \,.\, \cos \left| \overline{nt-C} \right| + \ q \,.\, \cos \left| \overline{nt-2Nt+C} \right| + \ r \,.\, \cos \left| \overline{3nt-2Nt-C} \right| + \ s \,.\, \cos \left| \overline{3nt-4Nt+C} \right| \end{array} \right\},$$

where, in each line, the third and fourth terms are much inferior in magnitude to the first and second, and their products with the small terms of the Factorial Equations are to be neglected. By differentiating δl we obtain

$$\frac{d\delta l}{dt} = \left\{ \begin{array}{rr} +Pt \,.\, \cos \left| \overline{nt-C} \right| & +\cdot 8504 \,.\, Qt \,.\, \cos \left| \overline{nt-2Nt+C} \right| \\ +(P-p) \,.\, \sin \left| \overline{nt-C} \right| & +(Q-\cdot 8504 \,.\, q) \,.\, \sin \left| \overline{nt-2Nt+C} \right| \\ +2\cdot 8504 \,.\, Rt \,.\, \cos \left| \overline{3nt-2Nt-C} \right| & +2\cdot 7008 \,.\, St \,.\, \cos \left| \overline{3nt-4Nt+C} \right| \\ +(R-2\cdot 8504 \,.\, r) \,.\, \sin \left| \overline{3nt-2Nt-C} \right| & +(S-2\cdot 7008 \,.\, s) \,.\, \sin \left| \overline{3nt-4Nt+C} \right| \end{array} \right\},$$

$$\frac{d^2\delta l}{dt^2} = \left\{ \begin{array}{rr} -Pt \,.\, \sin \left| \overline{nt-C} \right| & -\cdot 7231 \,.\, Qt \,.\, \sin \left| \overline{nt-2Nt+C} \right| \\ +(2P-p) \,.\, \cos \left| \overline{nt-C} \right| & +(1\cdot 7008 \,.\, Q-\cdot 7231 \,.\, q) \cos \left| \overline{nt-2Nt+C} \right| \\ -8\cdot 1247 \,.\, Rt \,.\, \sin \left| \overline{3nt-2Nt-C} \right| & \\ +(5\cdot 7008 \,.\, R-8\cdot 1247 \,.\, r) \,.\, \cos \left| \overline{3nt-2Nt-C} \right| & +(5\cdot 4016 \,.\, S-7\cdot 2944 \,.\, s) \,.\, \cos \left| \overline{3nt-4Nt+C} \right| \end{array} \right\}.$$

It is readily seen that wherever p appears it is accompanied with a multiple of P: and so for q, r, s. Then, when P, Q, R, S, are found from the terms which multiply t, and are substituted in those which do not contain t, but which are easily solved, p, q, r, s, will be directly deduced from them. We may therefore, in the first instance, omit entirely the terms independent of t. We proceed to substitute the terms depending on t in the factorial equation.

External force, representing the physical assumptions; or First Side of the Factorial Equation :—

$$= Bt \times \{+\cdot0083130 \cdot \sin\overline{|nt-C|} - \cdot0083130 \cdot \sin\overline{|nt-2Nt+C|} + \cdot0000166 \cdot \sin\overline{|3nt-2Nt-C|} - \cdot0000166 \cdot \sin\overline{|3nt-4Nt+C|}\}.$$

Corresponding terms of the Second Side of the Factorial Equation, including only multiples of t :—

$$= \left\{\begin{array}{l} +\{+1\cdot007945+\cdot0428 \cdot \cos\overline{|2nt-2Nt|}\} \\ \qquad \times t \times \{+P \cdot \sin\overline{|nt-C|} \quad + \cdot Q \cdot \sin\overline{|nt-2Nt+C|} \quad +R \cdot \sin\overline{|3nt-2Nt-C|} \quad +S \cdot \sin\overline{|3nt-4Nt+C|}\} \\ +\{+\cdot000000+\cdot0278 \cdot \sin\overline{|2nt-2Nt|}\} \\ \qquad \times t \times \{+P \cdot \cos\overline{|nt-C|} + \cdot8504 \cdot Q \cdot \cos\overline{|nt-2Nt+C|} + 2\cdot8504 \cdot R \cdot \cos\overline{|3nt-2Nt-C|} + 2\cdot7008 \cdot S \cdot \cos\overline{|3nt-4Nt+C|}\} \\ +\{+\cdot999545-\cdot0075 \cdot \cos\overline{|2nt-2Nt|}\} \\ \qquad \times t \times \{-P \cdot \sin\overline{|nt-C|} - \cdot7231 \cdot Q \cdot \sin\overline{|nt-2Nt+C|} - 8\cdot1247 \cdot R \cdot \sin\overline{|3nt-2Nt-C|} - 7\cdot2944 \cdot S \cdot \sin\overline{|3nt-4Nt+C|}\} \end{array}\right\}.$$

On performing the multiplications in these lines, omitting the products of small terms of the first factors by the coefficients of R and S in the second factors, the Second Side of the Factorial Equation becomes

$$+\left\{\begin{matrix}+\cdot008400 \cdot P \\ -\cdot0123 \quad \cdot Q\end{matrix}\right\} t \cdot \sin\overline{|nt-C|} \quad +\left\{\begin{matrix}-\cdot0114 \cdot P \\ +\cdot2851 \cdot Q\end{matrix}\right\} t \cdot \sin\overline{|nt-2Nt+C|}$$
$$+\left\{\begin{matrix}+\cdot0392 \cdot P \\ -7\cdot1130 \cdot R\end{matrix}\right\} t \cdot \sin\overline{|3nt-2Nt-C|} \quad +\left\{\begin{matrix}+\cdot0359 \cdot Q \\ -6\cdot2832 \cdot S\end{matrix}\right\} t \cdot \sin\overline{|3nt-4Nt+C|}.$$

This is to be compared with the First Side of the Factorial Equation.

Solving the four equations formed by comparison of the coefficients of sines of similar arguments,

$$P = +B \times 1{\cdot}003, \qquad Q = +B \times {\cdot}011, \qquad R = +B \times {\cdot}005, \qquad S \text{ is insensible.}$$

These, it will be remembered, are the important part of the result of the investigation as giving those term-coefficients which increase with time.

We now proceed with the less important terms whose coefficients do not increase with time, but which represent perturbations recurring with similar form and magnitude at every recurrence of similar positions of the Sun and Moon.

On the First Side of the Factorial Equation there are no coefficients independent of t. And there is no doubt that, in general, the terms of the result for constant coefficients will be so small that the retention of the principal part of each factor on the second side will be sufficiently accurate. There is one exception to this; that for each argument there are two coefficients, whose difference enters importantly, as divisor, into the equation; and in the special instance of the coefficients of cos $\overline{|nt-C|}$, the magnitudes of those coefficients are so nearly equal, and their difference (or divisor of the equation) so small, that in order to produce an available result, a greater number of decimals must be retained in their values. Remembering that the terms multiplying t have been entirely cleared out, the equations now to be treated are contained in these two lines—

$$0 = \left\{ \begin{array}{rr} +1{\cdot}007945 \,.\, p \,.\, \cos \overline{|nt-C|} & +\, q) \,.\, \cos \overline{|nt-2Nt+C|} \\ +\ {\cdot}999545 \,.\, (2P-p) \,.\, \cos \overline{|nt-C|} + (1{\cdot}7008 \,.\, Q - {\cdot}7231 \,.\, q) \,.\, \cos \overline{|nt-2Nt+C|} & \\ +r \,.\, \cos \overline{|3nt-2Nt-C|} & +S \,.\, \cos \overline{|3nt-4Nt+C|} \\ +(5{\cdot}7008 \,.\, R - 8{\cdot}1247 \,.\, r) \,.\, \cos \overline{|3nt-2Nt-C|} + (5{\cdot}4016 \,.\, S - 7{\cdot}2944 \,.\, s) \,.\, \cos \overline{|3nt-4Nt+C|} & \end{array} \right\}.$$

Referring to the late investigations of the values of R and S, it will be seen that r and s are totally unimportant; even q is far too small to be recognised in observation. But p is more important. Its equation is

$-1{\cdot}999090.\mathrm{P}+{\cdot}008400.\ p=0$, or $p=-238\times\mathrm{P}$, which is sensibly equal to $-238\times\mathrm{B}$, or to $-238\times{\cdot}479\times\sin 1''\times\frac{1}{84}$; or in seconds of arc, $p=-1''{\cdot}36$.

The physical result of the investigation, as regards the terms whose coefficients depend on time, may be thus presented. The elevation above the fixed ecliptic of 1900, of that part of the moveable ecliptic of $1900+t$, near which the Moon happens to be at the time t, is $\mathrm{B}t.\sin|v-\mathrm{C}|$, or

$$\mathrm{B}t\times\{+\sin|nt-\mathrm{C}|+{\cdot}00574\,.\,\sin|nt-2\mathrm{N}t+\mathrm{C}|+{\cdot}00574\,\sin|3nt-2\mathrm{N}t-\mathrm{C}|\}.$$

Thus we have

Perturbation of the Moon at the time t, above the fixed ecliptic of 1900. $\}=\mathrm{B}t\times\{+1{\cdot}003\,.\,\sin|nt-\mathrm{C}|+{\cdot}011\,.\,\sin|nt-2\mathrm{N}t+\mathrm{C}|+{\cdot}005\,.\,\sin|3nt-2\mathrm{N}t-\mathrm{C}|\}$,

Elevation above the fixed ecliptic of 1900, of that part of the moveable ecliptic of $(1900+t)$ near which the Moon happens to be at the time t. $\}=\mathrm{B}t\times\{+\sin|nt-\mathrm{C}|+{\cdot}006\,.\,\sin|nt-2\mathrm{N}t+\mathrm{C}|+{\cdot}005\,.\,\sin|3nt-2nt-\mathrm{C}|\}$,

Excess of the first line or Perturbation of the Moon at the time t above the moveable ecliptic at the time t. $\}=\mathrm{B}t\times\{+{\cdot}003\,.\,\sin|nt-\mathrm{C}|+{\cdot}005\,.\,\sin|nt-2\mathrm{N}t+\mathrm{C}|\}$.

The last figures I believe to be purely accidental, arising from the use of decimals to a point almost beyond what they can bear. Rejecting them, the general result is—

(A.) The Moon's movements, in so far as they might be supposed to depend on coefficients increasing with time, are the same with reference to a disturbed ecliptic as they would be with reference to an undisturbed ecliptic. (The immediate relative action of planets upon the Earth and Moon is not considered.)

But there is also a term $-1''\cdot36 \cos |\overline{nt-\Omega}|$ in the Moon's latitude, with invariable coefficient, which may be expressed in the form

$$-1''\cdot36 \,.\, \sin |\overline{\text{Moon's longitude} - 82^\circ\ 50'}|$$

or

$$+1''\cdot36 \,.\, \sin |\overline{\text{Moon's longitude} - 262^\circ\ 50'}|$$

Comparing this with the assumptions at the beginning, we see that—

(B.) The movements of the Moon, as expressed by the ordinary theories, are to be referred, not to the moveable ecliptic, but to a plane which is inclined to the moveable ecliptic in an angle of $1''\cdot36$; the axis of inclination being at right angles to the axes of rotation of the moveable ecliptic on the fixed ecliptic, and the greatest depression taking place at longitude $172''\ 50'$.*

We may express the term

$$-1''\cdot36 \,.\, \cos |\overline{\text{Moon's longitude} - 172^\circ\ 50'}|$$

under the equivalent form

$$+1''\cdot35 \,.\, \cos \text{Moon's longitude} - 0''\cdot17 \,.\, \sin \text{Moon's longitude}.$$

Now, on referring to the discussion of the Greenwich lunar observations in the *Memoirs* of this Society, volume xxix. page 23, it will be seen that the observations give an equation in latitude

$$+1''\cdot93 \,.\, \cos \text{Moon's longitude} - 0''\cdot59 \,.\, \sin \text{Moon's longitude},$$

(an equation which was the source of anxious consideration to Professor Hansen and myself). I do not doubt that this equation is completely explained by the result of the preceding theory. The law of connection of the two terms is well in accord; the magnitude of the coefficients is not perfectly in agreement, but not so discordant as at first appears (since the coefficients from the observations are the results of observed coefficients, every one of which is far inferior in merit to the exhibited result). It may be well to examine severely the factors in the investigation cited above.

1881, *February* 1.

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* In the *Darlegung*, p. 118, Hansen arrives at the conclusion that, disregarding periodic inequalities, the Moon moves in an orbit having a constant inclination, "nicht auf der gleichzeitig stattfindenden Ecliptik, sondern auf der welche nahe drei Jahre vorher Statt fand, und deren Knoten mit der gleichzeitigen Ecliptik überdies um einen Quadranten zurück gewichen sind." See also p. 494 of the *Darlegung*, where reference is made to the equation in latitude obtained from the Greenwich Lunar Observations.—Ed.

A Reply to Mr. Christie's Paper "On Mr. Stone's Alterations of Bessel's Refractions." By E. J. Stone, Esq.

In the *Monthly Notices* of the Society, vol. xli. No. 3, January 1881, there appears a paper by one of our Secretaries, Mr. Christie, which renders a reply desirable.

I am of opinion that good, and not harm, results from the full discussion of differences of opinion on scientific points; but the facts must not be obviously strained on either side. I should not have considered it necessary to reply to Mr. Christie were it not that some statements, on page 95, have been seriously misunderstood by many readers; and that some others, on page 107, appear to me to be deficient in precision.

In my paper on Mean Refractions, vol. xl., No. 6, 1880 April, I brought forward—at the request of some Fellows of the Society—the result of a recomputation of the residual error for the group of stars, 43° to 46° 29′, which was originally given in my paper on Bessel's Mean Refractions in the *Monthly Notices* for November 1867. The following statements are extracted from my paper of April 1880, pp. 338 and 339:—

"It may probably avoid useless discussion if I state clearly all the corrections applied to the Greenwich results.

"First: The observations have been extracted from the Annual Catalogues of the Greenwich Observatory 1857 to 1865 inclusive.

"Secondly: I have not used the Reflexion Observations. These observations have been already combined with the direct observations in the determination of the Nadir Points and in the deduction of the R—D corrections, and the corrections thus deduced have been applied in the Greenwich volumes to the direct results. With this exception, all the results given in the Greenwich volumes have been used.

"Thirdly: I have assumed that the thermometer used in 1857 and part of 1858 read too high by six-tenths of a degree Fahrenheit.

"Fourthly: I have assumed that the barometer used in the reductions read too low by a hundredth of an inch.

"Fifthly: As some of the observations contained in the group (5) have a greater zenith distance than 82°, and these observations have had their refractions already decreased by $0{\cdot}0023 \times$ tabular refraction, I have therefore increased their refractions by the same amount to bring them back to the results which would have been obtained had Bessel's Refractions been used unaltered.

"Sixthly: The observations have all been reduced to an assumed colatitude 38° 31′ 21″·80.

"Seventhly: I have weighted the observations from the formula—

$$W = \frac{1}{2\epsilon^2 + \frac{e^2}{n} + \frac{e'^2}{n_1}}$$

assuming $2\epsilon^2 = 0{\cdot}04$, and extracting e and e' from my paper on Probable Errors in the *Monthly Notices* for 1869. The result obtained in 1867 was $2''{\cdot}19$ with a weight 28, or probable error $\pm 0''{\cdot}2$. The result which I now obtain is $2''{\cdot}10$ with a weight 24, or probable error of $\pm 0''{\cdot}2$. The two results are essentially identical, and it is clear that all the corrections now applied were applied in 1867."

I am unable to appreciate the grounds upon which Mr. Christie can ever have supposed that, in a discussion regarding the necessity for certain breaks of continuity in Bessel's Refraction adopted at Greenwich, I carried on that investigation without taking these breaks of continuity into consideration. But I certainly thought that the statements contained in my paper of April 1880 were sufficiently explicit. It appears, however, that I was mistaken, for on pp. 95 and 96 of Mr. Christie's paper in the *Notices* for January 1881, we may read as follows—

"Notwithstanding Mr. Stone's explicit statement in the *Monthly Notices* for April 1880, pp. 338, 339, of the corrections which he has applied in forming his last residual, there are several points to be cleared up—

"1. Corrections for error of thermometer and barometer are stated to have been applied to the observations in 1857 and part of 1858. Now, I do not find in the Greenwich 'Introductions' any observations in those two years which would fall in this group.

"2. The limits of Mr. Stone's last group are N.P.D. 43° to 46° 29′. I have not been able to find any observations in the Greenwich 'Introductions' between N.P.D. $45\frac{1}{2}°$ and $47\frac{1}{2}°$.

"3. The assumed co-latitude is stated to be 38° 31′ $21''{\cdot}80$, but in his paper of 1867 Mr. Stone, after applying a correction $-0''{\cdot}205$, obtains 38° 31′ $21''{\cdot}61$, which would imply an assumed co-latitude 38° 31′ $21''{\cdot}815$. The difference is trifling, but it must be remembered that Mr. Stone gives his residuals to a thousandth of a second.

"I call attention to (1) and (2), not in any spirit of criticism, but because there seems a possibility that as Mr. Stone extracted his results from the Greenwich Annual Catalogues, his last group may contain observations accidentally omitted from the Latitude Investigations in the Greenwich 'Introductions' which I used.†"

[Note, page 95: "† On examining the Greenwich Observations for 1857, I find that there are, as a matter of fact, several stars in the Annual Catalogue which, for some reason, are not included in the Latitude Investigation in the Introduction. Mr. Stone had not stated in his paper of 1867 that he had taken his

results from the Annual Catalogues, and I thought it more natural that he should have taken them from the Introductions where they are collected. Thus our data are not the same."]

But in continuation we find, page 96—

"As regards the residuals, there is a point of some importance on which I am at direct issue with Mr. Stone—viz. the estimated probable error of the result for the last group. Mr. Stone obtains a weight of 28 or 24 with a probable error o″·2, while with the same system of weights I obtain a weight 6·5, with a probable error of o″·4. I have for the last group taken the probable error of an observation above the pole as o″·5 and S.P. as 1″·5, and I have altogether 30 observations above pole and 17 below, which gives as an *inferior* limit to the probable error o″·38, on the supposition that they all referred to the same star. It is just possible that the additional observations included by Mr. Stone may account for the discrepancy.

"In any case there are, I believe, but few stars observed in this group; and where the same star—*Capella*, for instance—is observed every year on the same division of the circle, account ought to be taken of the probability of a systematic error affecting the series. Thus the weight ought not to be assumed to be the sum of the weights for the separate years. Unless Mr. Stone has followed this last and erroneous system, I hardly see how he can have got such a large weight and small probable error for the result of his group."

I do not profess to understand the real drift of these statements of Mr. Christie. But there is an uncomfortable look about the combination of this pointed reference to pp. 338, 339, of my paper in the *Notices* for April 1880, and Mr. Christie's paragraphs (1) and (2), which has misled many readers, and conveyed an impression which I am sure that Mr. Christie could never have intended.

It is clear, from the note given at the bottom of page 95, that Mr. Christie is quite aware that he has overlooked a number of Greenwich observations which have an important bearing on these questions of refraction. As a matter of fact, there are in the Greenwich volume for 1857 no less than seven different stars observed, which fall within the group N.P.D. 43°—46° 29′, with 30 observations above the pole and 28 below the pole, whose results are not given in the Latitude Investigation in the Introduction. It appears now that these observations, and some others, have been omitted by Mr. Christie from his discussions. It is clear, therefore, that all force is removed from the paragraphs (1) and (2), which prove no error or misstatement whatever on my part; and it is equally clear that Mr. Christie was not likely to obtain as small a probable error for the group as that obtained by me, because he had not made use of *one half of the materials available for the discussion.*

I greatly regret that Mr. Christie has thought it necessary to publish criticisms which appear to me to derive whatever force

they may have, in the minds of his readers, from misconceptions of his meaning and intention.

I now come to the consideration of the statements made by Mr. Christie on page 97.

Mr. Christie considers my statement, "that the accumulated evidence of the Greenwich circumpolar observations is against the accuracy of Bessel's Refractions unaltered," as "really quite misleading." If I thought that such was the case the statement should be at once withdrawn; but the statement appears to me perfectly correct after the best reconsideration which I can give it. In 1857 Mr. Main discussed the Greenwich observations (below 70° zenith distance) made during the years 1836 to 1854, both inclusive. The practical result of that discussion was that Bessel's Refractions were abandoned at, and below, 82° Z.D. and another set of Tables=Bessel's Tables (1−0·0023) used for zenith distances between 82° and 85°. At 85° this second set of Tables was replaced by a third=Bessel's (1−0·0049). I presume, therefore, that the Greenwich observations 1836 to 1854 are not represented within probable errors by Bessel's Refractions unaltered, or these large and arbitrary changes would not have been considered necessary in the reduction of the Greenwich observations.

My investigation, made in 1867, discusses the observations made at Greenwich 1857 to 1865. The residual errors for five groups with N.P.D—0° to 15°; 15° to 30°; 30° to 37° 40′; 37° 40′ to 43°; and 43° to 46° 29′—are as follows:—

	(1)	(2)	(3)	(4)	(5)
With Bessel's Refractions, A	+0″·240	+0″·196	+0″·570	+1″·054	+2″·190
With Bessel's (1−0·0053) R. B	+0·15	−0·10	−0·14	−0·08	+0·10
The weights are	130	88	31	54	28

From 1868 to 1876 the Greenwich observations were reduced with Bessel's Refr. (1−0·0053), and the residual errors, with the colatitude determined, as it should be, from the observations themselves, but without attributing any weight whatever to the two last groups, which are chiefly affected by errors in the adopted refractions, are as follows:—

	(1)	(2)	(3)	(4)	(5)
C	+0″·15	−0″·22	+0″·05	−0″·31	+0″·22.

In this case the last two groups contain observations to 85° and below 85° respectively. Now, I have no hesitation in saying, no practical astronomer with the residual errors B and C before him, and informed that they contain the excess of N.P.D. below pole over N.P.D. above pole, for groups extending, in the case

of B to 85° Z.D., and in the case of C to below 85° Z.D., would, if in ignorance of what refraction tables had been used, express any other opinion than that the agreement was remarkably close, and proved the substantial accuracy of the mean refractions adopted. These are the facts upon which my statement was based, and I cannot recall it. But Mr. Christie refers to his paper (*Memoirs*, vol. xlv. p. 161) as affording some evidence against these diminished refractions and in favour of Bessel's Refractions unaltered. Let us see what this evidence amounts to. Of the four periods discussed by Mr. Christie, the observations made 1840–1847 are not extended below zenith distance 78°, and in such a discussion Mr. Christie was hardly likely to obtain any positive evidence against the refractions adopted for the determination of the colatitude. With respect to the periods 1851–1861, 1862–1865, Mr. Christie has unfortunately grouped together observations reduced with Bessel's Refractions and observations reduced with refractions obtained by throwing away from Bessel's Refractions quantities varying from 0″·9 to over 2″. And it now appears that he has omitted from the discussion altogether a large portion of the most valuable material, valuable so far as questions of refraction are concerned, which is available in the Greenwich volumes. I consider, therefore, that Mr. Christie's work requires revision throughout.

And here I must call attention to what, after giving the matter a careful consideration, appears to me a serious fallacy which invalidates Mr. Christie's work upon these questions of refractions. The refractions which he uses for comparison with the results obtained, 1868 to 1876, are not in reality Bessel's Refractions. They are Bessel's Refractions unaltered down to 82° zenith distance. At that zenith distance the error of Bessel's Refractions has apparently become sufficiently large to be distinctly put in evidence, and to be separated from the systematic instrumental errors to which all astronomical results are liable. At 82° a quantity of about 0″·9 is thrown away from Bessel's Refractions, and a new set of Tables used from 82° to 85° Z.D. At 85° Z.D. the error has apparently again become sufficiently large to be put in evidence, and the refractions are at 85° Z.D. a second time arbitrarily diminished by about 2″·9 from Bessel's Refractions, and a third set of tables used below 85° Z.D. Need I point out that the more conclusively it can be shown that the observations are represented by refractions thus computed, the more certain it is that *Bessel's Refractions unaltered* do not and cannot represent the Greenwich observations.

I know nothing about the errors of Bessel's Refractions except what I can gather from the observations. If these large corrections and abrupt breaks at 82° and 85° are not required, they should not be applied; but if they are required, it is useless to contend that Bessel's Refractions unaltered represent the Greenwich observations. And surely it is unnecessary for me to point out, if Bessel's Refractions are too great by 0″·9

at 82° Z.D. and the error is increasing in the same direction to 85° Z.D., that Bessel's Refractions cannot be without sensible error at zenith distances less than 82°. The error cannot change *per saltum* at 82°.

If the errors of Bessel's Refractions are such quantities as those given in my paper of April 1880, viz. +0″·4 at 70°, 0″·16 at 76°, 0″·28 at 78°, they are hardly likely to be detected from a general discussion of observations limited to an arc of 78°, when the observations are affected, as all the observations with which I am acquainted are affected, with systematic instrumental errors of at least equal or larger amounts.

With respect to what Mr. Christie, on page 104, calls my "*tinkering*" of Bessel's theory. What I have done is as follows:— Bessel's theory has two constants to be determined from observation. He first tried to include the refractions from the zenith to the horizon under one general investigation, and the errors of his theory were not, as a matter of fact, very great. But it was clear to Bessel that his refractions were too large at 85° by about 2″; and he recommended that for greater zenith distances than 85° the results of his theory should be replaced by refractions deduced more directly from observation. Now in the *Notices* for April 1880, I pointed out, and I think that any one who has had much experience in such investigations will recognise the importance of the point, that when Bessel found it necessary to abandon his attempt to include all the refractions from the zenith to the horizon in one investigation, and restricted the theory to a smaller arc than 90°, he should have re-determined both the constants of his theory from observations included within the restricted arc, and not allowed one of the constants to be strained by attempts to represent observations near the horizon, attempts which had confessedly not been successful.

What I think Bessel should have done when he found his refractions too large at 85° Z.D., I have done, and the results are given in the *Notices* for April 1880. I have, as there clearly stated, assumed the error at 85° Z.D.=2″·87. This quantity may be, for aught I know, too large from the accidental run of the systematic errors of the Greenwich observations. But I have no good reasons for supposing such to be the case. If the assumed error be sensibly correct, then the corrections given by me for Bessel's Refractions at smaller zenith distances are entitled to be received with confidence; but if the error 2″·87 is much too large for Z.D. 85°, then the errors given for the smaller zenith distances will also be slightly too large. If this be *astronomical* "*tinkering*," all I can say is, that I am well content to be an "Astronomical Tinker."

With respect to Mr. Christie's objections to my use of the co-latitude determined from the observations 1868–1876 in exhibiting the residual errors of these observations, I am quite unable to follow Mr. Christie's reasoning. If the method, adopted at Greenwich 1871–1875, of applying a correction of 0″·6 to

0″·8 to the nadir determinations before using them, in combination with the reflexion observations, for the determination of zenith points was perfectly satisfactory, then the co-latitudes determined from the observations should be perfectly satisfactory, and should be used, as I have done, and not rejected, as Mr. Christie has done. If this method of treating the not inconsiderable discordance between the nadir points, although the best practical method of meeting a confessed difficulty, is not altogether satisfactory, then the use of the co-latitudes determined from the observations themselves should, to a great extent at all events, correct any outstanding constant error; but, to secure this, the co-latitude deduced from the observations which are affected with the same constant error must be used. Mr. Christie's argument, on page 108, although brought forward to prove me wrong, affords, in my judgment, the most satisfactory evidence that could be adduced in favour of the course which I have adopted. "The apparent change in the co-latitude on which Mr. Stone remarks has nothing to do with this correction (*i.e.* constant difference between the nadir points with reflecting eyepiece and reflexion stars), but is simply due to a change in the system of weights given to the close circumpolar stars and to the increased number of observations of wide circumpolars. It did not begin in 1871, when the correction to the nadir observation was first applied, but in 1872, when special attention was directed to the observation of wide circumpolars. Previous to that year the large weight given to the four close circumpolars made the value of the co-latitude virtually depend on them; and if this system had been continued the same value would have been obtained in subsequent years. The four close circumpolars give for the period 1868–1876, 38° 31′ 21″·95 with Bessel's Refractions, and 38° 31′ 21″·70 with the diminished refractions; whereas the stars about N.P.D. 20° give about 38° 31′ 21″·50 and 38° 31′ 21″·17. It is simply the admission of these latter stars to a fair share in the result which has produced an apparent change. The apparent change does not justify Mr. Stone in using two different co-latitudes in the periods 1857–1865, and 1868–1876." Surely, if the smaller co-latitude given by these observations, 1868–1876, is due solely to a better distribution of the weights than that formerly adopted, Mr. Stone is, at all events, justified in using the co-latitude deduced from these observations, and Mr. Christie is certainly not justified in rejecting it in favour of a value which would have been obtained with a more imperfect system of weights. But we are really concerned in these questions with the effective co-latitude (*i.e.* the actual differences of readings on the circles between the mean adopted nadir point, or zenith point, and the polar point) and not with the real colatitude of the Greenwich Observatory, even should there, as a matter of fact, be any sensible difference between them.

I must now refer to some statements of fact by Mr. Christie,

on page 107, which I do not think are made with the precision which is desirable.

"In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^r of the microscope micrometers, and thus was obtained that accordance of separate results which is so dear to some astronomers, but which is not always conducive to the elimination of systematic errors. The additional wear resulting from this increased use of one part of the screws gave rise to a discordance between the nadir observations and observations of stars which were taken at all parts of the screws. This discordance is shown in the years 1869 and 1870 by the large constant term in R—D; in subsequent years a correction was applied to each nadir observation derived from the mean results for the preceding year. In 1875, being dissatisfied with the large correction for runs virtually required to reduce the nadir observation to the mean of observations of stars which would fall at about $2^r{\cdot}5$, I suggested that the nadir observation should be taken at from $2^r{\cdot}5$ to $3^r{\cdot}5$, and it was then found that the correction to the nadir observation had changed from $-0''{\cdot}8$ to $+0''{\cdot}6$. This threw suspicion on the micrometer screws, and their errors were examined, as explained in my paper of 1876. The result was to show that corrections were required ranging from $-1''{\cdot}4$ at $1^r{\cdot}3$ to $+0''{\cdot}8$ at $2^r{\cdot}3$, the correction at $0^r{\cdot}0$ being $-0''{\cdot}9$, corresponding very closely with the correction to the nadir observation $-0''{\cdot}8$. Further, observations taken for another purpose in 1868 showed that the errors of the micrometer screws were sensibly the same at that time, with the exception of the portion about $0^r{\cdot}0$, which had suffered extra wear since 1868 from a preponderance of some 4,000 observations taken at this part."

If this statement means that in 1868 the practice was introduced of taking the nadir points near the readings $0^r{\cdot}0$ of microscope A, then it is certainly not correct. If it simply means that, to avoid very large corrections for run, the nadir points were generally taken, after 1868 Feb. 1, at readings between $0^r{\cdot}0$ and $1^r{\cdot}5$, or over a range of the screws with which more than a quarter of all the observations made with the circle would be made, then the statement may be accepted as substantially accurate. But, in this case, it will fail to account for the serious wear of the screws near $0^r{\cdot}0$, which Mr. Christie supposes to have taken place shortly after the introduction of the change. I have taken the trouble to examine the Greenwich observations 1868 and 1869, and I find as follows. From 1868 Feb. 1 to 1869 Dec. 31 there were only 108 determinations of the nadir point at readings of microscope A less than $0^r{\cdot}2$, whilst there were 72 determinations made at readings of microscope A above $1^r{\cdot}0$. The largest reading is $3^r{\cdot}0$, but there are several as large as 2^r. Whatever, therefore, may have been the cause of the state of the screws near $0^r{\cdot}0$ in 1876, it certainly was not caused by the additional 4,000 observations made near $0^r{\cdot}0$ at my suggestion.

I left for the Cape in 1870. The last discussion of the R—D which I could have had the advantage of seeing was that for 1868. But if the screws were worn, in the way indicated by Mr. Christie in his paper of 1876 November, so that the error changed from $-1''.2$ at $-\bar{1}^r.9$ to $-0''.9$ at $0^r.0$ and to $-0''.5$ at $0^r.1$, then the values of the *runs* determined for readings somewhere near $0^r.0$ to $4^r.8$ or $4^r.9$ must have been seriously affected, and have depended very much indeed upon the exact reading from which the run was actually commenced. From the form of the curve of errors given in Mr. Christie's paper 1876, I should have supposed that the runs had not been corrected for errors of screws before their application, and that this oversight had caused the errors of the screws to be *apparently* nearly the same for the large readings near $4^r.8$ and for the small readings, from which the run was generally started; but I am persuaded that such an oversight could not possibly have been made by Mr. Christie in a work of importance.

The correction of the Greenwich Results for these assumed errors of screws cannot be completed without a correction of the runs, of the readings of the screws employed in making any particular observation, and of the readings included in the adopted zenith point determination; and as the actual readings from which the runs are determined are not published, I have been unable to correct the Greenwich Results for errors of screws. But fortunately such corrections are certain, in mean results, to be very small; and in questions of errors of our refraction tables at such zenith distances as 82° to 85° and 85° to 87° with which I was concerned, such errors are altogether unimportant.

As discussions involving in any degree personal questions have a tendency to obscure the real points at issue, and as I have neither the time nor inclination for mere verbal discussions, I will state, as clearly as I can, what I maintain and what I admit with respect to these questions of refraction.

1. I maintain that the Greenwich circumpolar observations are not represented within errors of observation by *Bessel's Refractions unaltered* from 0° to 85° Z.D. Bessel himself recommended their abandonment below 85° Z.D.

2. I maintain that Bessel's Refractions $(1-0.005)$ do, whether accidentally or not I cannot say, represent, within errors of observation, the Greenwich circumpolar observations 1857 to 1876.

The co-latitude must of course be determined with the same set of refraction tables.

3. I infer, from the general results of Main's discussion of the observations 1836–1854, that if these observations be treated *as a whole*, and no abrupt breaks in continuity of the refractions used be admitted, then they would be represented either within, or very nearly within, errors of observation, by the use of Bessel's Refractions $(1-0.005)$. This I think is shown in my paper of April 1880.

4. I point out that the observations 1836–1854 and the observations made 1857 to 1876 at greater zenith distances than 85° are closely represented also by these diminished refractions.

5. I freely admit that a comparison between the Cape observations, which I have recently put into the hands of astronomers, exhibits a most astonishingly close agreement, for moderate zenith distances, with the Greenwich Results when the observations are reduced with Bessel's Refractions unaltered; but the observations show that Bessel's Refractions at 82°, and certainly at 85°, are much too large for the Greenwich observations.

6. I maintain that any refraction tables, to merit acceptance, should satisfy the known conditions without any abrupt breaks of continuity over the arc for which they are used.

7. I fully admit that the solution obtained by me in 1867, which satisfied the then known conditions, does not satisfy *all* the conditions brought forward by my Cape work.

8. I maintain that Bessel's Refractions unaltered fail to represent either of the necessary conditions to 85° Z.D.

9. I believe that by a slight but necessary correction of Bessel's theory, brought forward in the *Notices* for April 1880, the conditions, so far as I know them at present, are fulfilled.

I therefore, of course, much prefer the solution of 1880 April to that of 1867 November. The one satisfies one set of conditions, whilst the other satisfies two. A complete solution must satisfy all the conditions of a problem. A solution to be worth acceptance even for a time, and until replaced by something better, should satisfy all the known conditions. But, if it does satisfy the known conditions, it is worth acceptance until facts inconsistent with it have been distinctly put in evidence. It must then, of course, be modified or abandoned. I know of no method by which progress, and not stagnation, can be secured, except by thus testing and modifying our theories as fresh data are brought forward.

What I have objected to, and do most strongly object to, is that directly some fresh conditions for testing these questions of refraction were brought forward by my publication of the Cape Catalogue 1860, some astronomers appeared to rush to the conclusion that these conditions were alone worth consideration, and that all our previous work was wrong; and (overlooking, or at least not giving any sufficient prominence to, the fact that the Greenwich observations, which agreed so well with the Cape observations, agreed at considerable zenith distances only by a virtual abandonment of Bessel's Refractions) they upheld the accuracy of Bessel's Refractions unaltered, and attempted to explain away the force of the evidence slowly accumulated at Greenwich by the labour of years; and, whether intentionally or not, certainly created a false impression that I had committed some serious mistake in my paper of 1867. No mistake, so far as I am aware of, was made; the whole of the available data were represented by the solution obtained.

Note on a Paper by Mr. Denning in Monthly Notices, No. 2, vol. xli. By J. L. McCance, Esq.

Mr. W. F. Denning has a paper on "*Saturn* in October 1880" in the *Monthly Notices*, vol. xli. No. 2. I only wish to say a very few words with reference to one subject, namely, the "curious figure" of the planet which Mr. Denning describes. I beg to corroborate his remarks as to this odd appearance. I have observed it on several occasions, and ascribed it much to the same causes as does Mr. Denning. It follows, I imagine, that this "square-shouldered" aspect must be more or less prominent as the arrangement of the belts varies from year to year; and this may be the reason of the observations of it being comparatively rare. It would be interesting to learn if other observers have noticed this appearance near the late Opposition; for myself I did not observe *Saturn* near the Opposition, but I have a Note on Nov. 21, $11^h\ 20^m$, that the "square-shouldered" aspect was very plain then.

Putney Hill, S.W.
1881, *Jan.* 24.

Comparative Observations of Disappearance and Reappearance of Jupiter's Satellites. By the Rev. S. J. Johnson.

The following observations during the late apparition of *Jupiter* were taken with a $3\frac{1}{4}$ in. and $2\frac{1}{4}$ in. telescope placed side by side, each charged with a power of 100, for the purpose of noting the difference caused by one inch of aperture in the earliest and latest visibility of the eclipsed satellites. The time was obtained by three or four single altitudes of the Sun taken with the sextant. The immersions and emersions, as might be expected with a small instrument, correspond, in the case of the first satellite, pretty closely with the predicted times given in the "Nautical Almanack;" the result of observations on Nov. 4, 27, and Dec. 20 according with the latter within 4 sec., 2 sec., and 1 sec. respectively.

1880, Sept. 3	III.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	11	16 28
			$3\frac{1}{4}$ "	11	17 52
7	II.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	11	11 48
			$3\frac{1}{4}$ "	11	12 39
8	I.	Ec. D.	With $2\frac{1}{4}$ inch, last visibility	13	15 28
			$3\frac{1}{4}$ "		

Planet in slight fog. On removing my eye to the larger instrument, I found it was not in sight.

Nov. 3	II.	Ec. R.	With 3¼ inch, first visibility	10	28	2
			2¼ „	10	28	39
Nov. 4	I.	Ec. R.	With 3¼ inch, first visibility	6	46	39
			2¼ „	6	46	55
Nov. 20	I.	Ec. R.	With 3¼ inch, first visibility	5	7	19
			2¼ „	5	7	29
Nov. 27	I.	Ec. R.	With 3¼ inch, first visibility	7	2	58
			2¼ „	7	3	18
Nov. 28	II.	Ec. R.	With 3¼ inch, first visibility	7	33	23
			2¼ „	7	33	39
	III.	Ec. D.	With 2¼ inch, last visibility	11	44	48
			3¼ „	11	45	35

Planet in slight haze.

Dec. 20	I.	Ec. R.	With 3¼ inch, first visibility	7	19	33
			2¼ „	7	19	48
Dec. 30	II.	Ec. R.	With 3¼ inch, first visibility	7	16	55
			2¼ „	7	17	13
1881, Jan. 6	II.	Ec. R.	With 3¼ inch, first visibility	9	54	2
			2¼ „	9	54	32
Jan. 31	II.	Ec. R.	With 3¼ inch, first visibility	7	3	29
			2¼ „	7	4	23

Abbenhall Rectory, Gloucester,
1881, *March* 10.

Observations of the Phenomena of the Satellites of Jupiter, with a few Transits of the "Red Spot;" also a few observations of the brighter Satellites of Saturn, made at the Observatory of Mr. Edward Crossley, F.R.A.S. By Mr. J. Gledhill, F.R.A.S.

The instrument used is the equatorial refractor of 9⅓-in. aperture. In the observations of the phenomena of *Jupiter's* satellites the full aperture and powers 240 and 470 were used. The transits of the "red spot" and the phenomena of *Saturn's* satellites were observed with the Simms micrometer, powers 280 and 460; in the former observations the equatorial diameter of *Jupiter* was measured, the webs set at half the value, and one kept on the eastern or western limb of the planet at right angles to the planet's equator: in the latter a web was placed at right angles to the plane of the ring, and at one of its ends; or the semi-diameter of the ball was found, and the webs placed as in the transits of the "red spot."

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation.	Mean Solar Time from N. A.	Remarks.
			h m s	h m s	
1880, Aug. 31	II.	Oc. R. External contact	12 57 0	12 58 0	Good definition.
Sept. 3	(*a*) III.	Ec. D. Began to fade	11 14 36	11 17 25	Good definition.
		Disappeared	11 18 42		
7	(*b*) II.	Ec. D. Began to fade	11 10 0		Definition bad.
	(*c*)	Half gone	11 11 20	11 11 1	
	(*d*)	Just disappeared	11 12 50		
17	I.	Ec. D. Fading	9 36 0	9 38 47	Much cloud.
		Invisible	9 38 30		
18	I.	Sh. E. Just off	9 8 0	9 9 0	Wind and clouds.
26	I.	Oc. R. External contact	8 31 0	8 31 0	Much motion.
Oct. 2	(*e*) II.	Ec. D.	8 12 53		Definition poor.
	(*f*)		13 53	8 14 6	
	(*g*)		15 38		
	I.	Sh. I. Notch first seen	10 44 0	10 43 0	Bad definition.
	I.	Tr. I. First contact	10 49 0		
		Bisection	10 52 0	10 51 0	
		Inner contact	10 53 0		
	II.	Oc. R. Last contact	11 5 0	11 5 0	
Oct. 3	(*h*) I.	Ec. D. Disappearance	7 57 6	7 57 26	

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Oct. 3	(*i*) I.	Oc. R. First seen	10 12 0		
		Half off	10 12 30	10 15 0	
		Ext. contact	10 14 30		
9	(*j*) III.	Ec. R. First seen	9 45 12	9 50 19	Good definition.
	(*k*) II.	Oc. D. First contact	10 40 0		
	(*l*)	Bisected	10 42 0	10 40 0	
	(*m*)	Disappearance	10 44 0		
10	(*n*) I.	Oc. D. First contact	9 44 0		
	(*o*)	Bisected	46 30	9 46 0	
	(*p*)	Inner contact	48 0		
19	(*q*) I.	Ec. R. First seen	8 26 28	8 26 53	Good definition.
Nov. 1	II.	Tr. I. First contact	11 58 0		Good definition.
	(*r*)	Bisected	11 59 0	12 0 0	
		Second contact	12 0 0		
	(*s*) I.	Tr. I. First contact	12 14 0		
		Second contact	12 16 0	12 13 0	
3	II.	Oc. D. First contact	6 28 0	6 30 0	Very bad sky.
		Disappearance	6 34 0		
	I.	Tr. I. First contact	6 37 0	6 39 0	Violent boiling.
		Second contact	6 44 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Nov. 3	III.	Tr. I. First contact	6 51 0	6 54 0	
		Second contact	7 5 0		
	I.	Sh. I. Just within	7 21 0	7 19 0	
	I.	Tr. E. Inner contact	8 48 30		Good definition now.
		Bisected	8 50 30	8 52 0	Good observations; the last thought the best.
		Outer contact	8 52 0		
	(*t*) III.	Tr. E. Inner contact	9 8 0		Uncertain.
	(*u*)	Bisected	9 15 0	9 21 0	Fairly good.
	(*v*)	Just off	9 22 0		Fairly good.
	I.	Sh. E. Inner contact	9 27 0	9 33 0	Definition not so good.
		Just off	9 32 0		
	III.	Sh. I. Bisected	9 42 0	9 40 0	
		Just within	9 44 0		
	II.	Ec. R. First seen	10 25 30	10 28 43	
	III.	Sh. E. Inner contact	12 5 0		
		Half off	12 8 0	12 11 0	
		Just off	12 9 30		
Nov. 4	I.	Ec. R. First seen	6 46 25	6 46 42	Bad sky.
17	I.	Tr. I. First contact	10 13 0	10 12 0	Good sky; good observation.
		Fully on disk	10 16 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Nov. 18	I.	Oc. D. First contact	7 27 0	7 27 0	Good observation.
		Disappearance	7 29 0		
	I.	Ec. R. First seen	10 38 7	10 38 6	Good observation.
19	I.	Sh. I. Just on	5 40 0	5 39 0	
	II.	Tr. I. First contact	5 50 0	5 54 0	Rather uncertain.
		Bisection	5 53 0		Good.
		Second contact	5 55 0		Good.
	I.	Tr. E. Inner contact	6 47 0		Not so good.
		Bisection	6 50 0	6 53 0	Good.
		Ext. contact	6 53 0		Best observation.
	I.	Sh. E. Inner contact	7 47 0		Good.
		Bisection	7 49 0	7 53 0	Fair.
		Just off	7 51 0		Good.
	II.	Sh. I. Half on	7 54 0	7 53 0	Fair.
			7 56 0		Good.
	II.	Tr. E. Inner contact	8 24 0		Fair.
		Bisection	8 27 0	8 34 0	Good.
		Just off	8 31 0		Good.
	II.	Sh. E. Inner contact	10 28 0	10 33 0	
		Just off	10 31 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N.A. h m s	Remarks.
Nov. 20	I.	Ec. R. First seen	5 7 1	5 7 7	Good observation.
21	II.	Ec. R. First seen	4 57 4	4 58 4	Good.
25	I.	Oc. D. First contact	9 13 0		Good definition.
		Bisection	9 15 0	9 16 0	
		Disappearance	9 17 0		
26	I.	Tr. I. First contact	6 27 0		Good.
			6 29 30	6 27 0	Fair.
			6 31 0		Good.
	I.	Sh. I. Bisection	7 34 0	7 34 0	
		Just within	7 36 0		
	II.	Tr. I. First contact	8 13 0	8 13 0	Clouding.
	II.	Tr. E. Inner contact	10 51 0		
		Half off	10 54 0	11 0 0	
		Just off	10 57 0		
27	I.	Ec. R. First seen	7 2 32	7 2 56	
28	II.	Ec. R. First seen	7 34 20	7 34 3	
	III.	Oc. R. Outer contact	9 29 0	9 35 0	
Dec. 2	I.	Oc. D. First contact	11 4 0		Fair sky.
		Bisection	11 6 0	11 6 0	
		Last seen	11 8 0		

Day of Observation.	Satellite.	Phenomenon.	Mean Solar Time of Observation. h m s	Mean Solar Time from N. A. h m s	Remarks.
Dec. 11	I.	Oc. D. First contact	7 25 0	7 26 0	Bad definition.
		Last seen	7 28 0		
	I.	Ec. R. First seen	10 54 43	10 54 43	
12	I.	Tr. E. Just off	6 54 0	6 52 0	Bad sky.
	II.	Oc. D. Just gone	7 37 0	7 36 0	
14	II.	Sh. E. Inner contact	7 46 0	7 50 0	Bad sky.
1881, Jan. 13	II.	Oc. D. Outer contact	7 14 0		Good.
		Half gone	7 16 0	7 14 0	Fairly good.
		Gone	7 18 0		Good.

Notes.

(*a*) Power 227 on the micrometer; probably the light began to fade a little (a few seconds) later than $11^h\ 14^m\ 36^s$.
(*b*) and (*c*) Uncertain. (*d*) Good; power 500. (*e*) Fair. (*f*) Uncertain. (*g*) Good; power 282.
(*h*) The satellite was fully 2^m in disappearing; it was very near the limb of *Jupiter*.
(*i*) Time uncertain. (*j*) The satellite was fully 3^m in attaining its full brightness. (*k*) and (*l*) may be 1^m in error.
(*m*) Good. (*n*) and (*p*) Good. (*o*) Uncertain. (*q*) The satellite was about 4^m in attaining full brightness.
(*r*) Uncertain. (*s*) Not later than this. (*t*) Uncertain. (*u*) and (*v*) Fairly good.

Transits of the "Red Spot" of Jupiter.

1880, Aug. 31.

Preceding end:

h	m	
13	10	not up.
13	15	central?
13	20	past.

Centre:

h	m	
13	40	not up yet.
13	43	central?

Clouds passed and prevented further observation.

1880, Sept. 3. Definition bad.

Preceding end:

h	m	
10	55 ±	judged central.

Centre:

h	m	
11	10	not up yet.
11	14	central.
11	18	certainly past.

Following end:

h	m	
11	38	not quite up.
11	40	central.
11	44	past.

1880, Sept. 18. Wild night: no definition.

Following end:

h	m	
9	0	not up.
9	10	certainly past.

1880, Oct. 2. Definition not good.

Preceding end:

h	m	
9	27	not up.
9	34	just passed.

Centre:

h	m	
9	54	not up.
9	59	central.
10	4	just past.

Following end:

h	m	
10	25	not up.
10	29	central.
10	32	past.

1880, Oct. 9. Good sky.

Preceding end:

h	m	
10	5	not up yet.
10	7	to 10^h 11^m uncertain.
10	13	central.
10	15	past.

Centre:

h	m	
10	40	not up.
10	42	to 10^h 44^m uncertain.
10	47	past.

Following end:

h	m	
11	10	not up.
11	12	not up.
11	13	to 11^h 14^m uncertain.
11	17	certainly past.

1880, Oct 11.

Preceding end:

h	m	
11	47	not up.
11	48	to 11^h 50^m uncertain.
11	52	judged central.
11	55	past.

Centre:

h	m	
12	18	not up.
12	20	to 12^h 23^m uncertain.
12	25	central.
12	27	probably just past.
12	29	certainly past.

Following end:

h	m
13	17 not up.
13	23 to 13^h 25^m uncertain.
13	27 judged central.
13	30 past.

1880, Oct. 19.

Preceding end:

h	m
8	23 not up.
8	24 uncertain.
8	25 central?
8	28 central?
8	30 past.

Centre:

h	m
8	50 not up.
8	53 central?
8	55 central?
8	56 central?
8	58 certainly past.

Following end:

h	m
9	21 not up.
9	23 to 9^h 25^m central?
9	27 certainly past.

1880, Oct. 21. Definition very bad.

Preceding end:

h	m
10	5 not up.
10	7 to 10^h 10^m central?
10	12 past.

Centre:

h	m
10	36 central?

Following end:

h	m
11	1 to 11^h 3^m central?
11	5 past.

1880, Nov. 19.

Preceding end:

h	m	
8	45	not up yet.
8	50	not up yet.
8	58	central?
9	0	central?
9	3	past.

Centre:

h	m	
9	20	not up.
9	22	to 9^h 24^m uncertain.
9	26	central.
9	30	past.

Following end:

h	m	
9	45	not up.
9	50	up?
9	52	up?
9	55	past.

1880, Nov. 20. Good definition.

Centre:

h	m	
5	13	not up.
5	15	up?
5	16	up?
5	18	central.
5	19	central.
5	20	past.

1880, Nov. 21. Definition not good.

Centre:

h	m	
6	50	not up.
6	55	central?
7	0	central.
7	5	past.

Following end:

h	m	
7	20	probably central.

1880, Dec. 18. Stormy; planet seen at intervals.

Preceding end:

	h	m	
Central at	7	50	±.

1881, Jan. 14. Definition good till the centre had passed, then very poor.

Preceding end:

h	m	h	m	
5	10	to 5	15	central; not up at
5	5,	past at 5^h 20^m.		

Centre:

It passed between 5 h 40 m and 5 h 45 m.

Following end:

It passed between 6 h 10 m and 6 h 15 m.

Observations of Saturn's Satellites.

1880, Sep. 3. Full aperture, $9\frac{1}{3}$ inches, always used. Good definition.

Tethys at inf. conjunction with the following end of the ring:

h	m	
11	55	not yet up.
12	3	on the web at right angles to axis of ring.
12	8	certainly past.

The satellite is steadily visible in the dark field: a little illumination increases the visibility.

The mist thickened and rendered the conjunction with the centre of the ball uncertain. It occurred between 15^h 30^m and 15^h 40^m.

1880, Oct. 23.

Rhea in conjunction (south) with the centre of the ball:

h	m	
9	40	not up.
9	45	uncertain; probably up.
9	50	uncertain; probably up.
10	0	certainly past.

Powers 240 and 470.

1880, Nov. 1.

Rhea in conjunction (south) with centre of ball:

h	m	
10	30	not yet up.
10	32	uncertain; probably up.
10	32	uncertain; probably up.
10	40	just past.

Full red illumination used: good sky.

1880, Nov. 3.

Dione in conjunction (south) with east end of ring:

h	m	
8	50	on the line.
9	0	certainly just past.

Power 240. Definition not good. At 12^h the satellite was an easy object with the aperture ($9\frac{1}{3}$ in.) reduced to 7 in.

Rhea in conjunction (north) with east end of ring:

h	m	
12	5	not up.
12	7	up?
12	10	certainly past.

1880, Nov. 19. Good sky. *Enceladus* was seen steadily at 7 P.M.

Rhea in conjunction (south) with preceding end of the ring:

h	m	
7	15	not up.
7	17	up.
7	20	certainly past.

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*Second Catalogue of Radiant Points of Meteors.**

By Edwin F. Sawyer, Esq.

The following list of Meteoric Radiant Points has been deduced from my observations comprising the recorded paths of about 900 out of a total of 1,156 shooting stars seen during the last two years (1879–80) at Cambridgeport, Mass., U.S. The positions deduced mainly consist of those of well-known meteor systems, although the radiant centres of a few strongly suspected and possibly new showers have been determined. A few positions deduced also serve to confirm meteor showers previously observed but once or twice, and needing such verification. Extreme care has been exercised in deducing the results, a certain weight being attached to each meteor mapped, showing the accuracy of the recorded path on a scale of one to four, and its corresponding value used in determining the different centres of

* First Catalogue published in the *American Journal of Science and Arts*, vol. xvii. June 1879.

radiation. The peculiarities of each individual meteor mapped, such as length of path, velocity, magnitude, whether accompanied by streaks or not, &c., have also been regarded in reducing the results, so that the positions as determined may be considered as accurate as the number of meteors mapped and the character of this class of observations will allow. Observations were taken nearly every fine night in the absence of the moonlight (with the exception of during the months of January and February 1879, and March 1880, when sickness prevented), but were confined principally to the evening hours between 7 and 12 P.M. A few morning watches were, however, maintained in April, August, October, November, and December for the appearance of the well-known meteoric showers active during these months.

The period of watching each evening varied from one to five hours in duration, and amounted to 221 hours in all. The observations were almost wholly confined to the eastern and north-eastern quarters of the sky. The showers have (as during my previous observations) been deduced from the material gathered from short periods of watching, the average duration of the showers being about four days. The results, therefore, give the observed duration and not the true period of each shower, as yet an uncertain element in the majority of showers now catalogued. The following table exhibits, 1st, the days each month on which observations were obtained; 2nd, the number of hours spent in watching; 3rd, the number of meteors recorded; 4th, the number of radiant points deduced; and 5th, the number of meteor tracks employed in determining the radiant centres.

Months and Days.	Hours.	No. of Meteors seen.	No. of Radiant Points deduced.	No. of Meteor Tracks used.
1879, January and February. None ...	0	0	0	0
March 18, 19	2	7	0	0
April 12, 14, 19–23, 26 ...	16	56	6	32
May 8, 9, 11	3	11	0	0
June 8, 13, 17, 18, 21, 23 ...	6	26	1	9
July 12–14, 17, 19	8	26	3	15
August 7, 10–12, 20	10	171	10	71
September 8, 9, 11, 14, 15, 17, 20	11	45	3	27
October 7, 13, 14, 19, 20 ...	13	77	11	54
November 7, 10, 15, 16 ...	11	60	5	31
December 7, 9, 12, 16	11	69	4	43
Total 49 days ...	91	548	44	282

Months and Days.	Hours.	No. of Meteors seen.	No. of Radiant Points deduced.	No. of Meteor Tracks used.
1880, January 2 5, 7, 8, 10	13	36	2	13
February 1, 6, 7, 8	6	12	2	6
March. None	0	0	0	0
April 5, 7, 8, 12, 30	8	17	0	0
May 2, 6, 8, 10, 11	6	13	0	0
June 29, 30	3	12	2	9
July 4, 7, 28–31	13	99	8	65
August 1, 6, 8, 9. 26, 28, 31 ...	16	172	9	79
September 1–4, 11, 24, 25, 28–30	19	64	5	37
October 1–3, 5, 6, 8, 9, 24, 25, 29, 31	27	113	10	53
November 1, 2, 11, 12, 25, 26, 29	18	68	2	17
December 3	1	2	0	0
Total 63 days ...	130	608	40	279
1879 ... 49 days	91	548	44	282
1880 ... 63 „	130	608	40	279
Grand Total ... 112 days	221	1156	84	561

The estimated magnitudes of the meteors seen were as follows:—

>1st.	=1st.	=2nd.	=3rd.	=4th and fainter.	Total.
76	142	201	280	457	1156

Of the meteors >1st mag. in brightness, there were 3 > ♀, 3= ♀, and 32 equal or exceeding ♃ in brilliancy. 79 of the 218 meteors = or >1st mag. in brightness were noted as exhibiting more or less colour, as follows:—8 were classed as deep orange, 39 orange, 4 yellow, 12 green, and 16 blue. The average length of path of the 912 meteors mapped was 9° 9′. The following list of stationary, or nearly stationary, meteors have been recorded during the past two years:

Stationary Meteors.

No.	Date.	Position.	Mag.	Weight.	Remarks.
1	1879, March 15	195°−5°	3	3	
2	19	198+7½	4	4	Slight movement, about ¼°
3	April 12	223−2	1	3	Very slight movement; visible 1ª
4	14	206−8	2	4	
5	14	205−2 to 204−1½	3	4	Path 1°
6	14	207+25	3	3	
7	21	238−7	4	3	
8	21	262+17	4	3	
9	22	253+5	1	3	

No.	Date.	Position.	Mag.	Weight.	Remarks.
10	1879, May 9	275° + 12°	2	3	1 sec.
11	9	225 + 3	1	3	Nearly stationary, path $\frac{1}{4}$°
12	June 8	259 + 11 to 259½ + 10	3	1	Path 1°
13	13	282½ + 20	4	3	
14	23	298 + 27½ to 298 + 26½	4	3	Path 1°
15	July 12	325 + 28	4	3	Slight movement, ½°
16	Oct. 13	11 + 40	4	3	
17	14	22 + 21	2	4	Nearly stationary, yellow.
18	19	18 + 29	5	3	
19	19	92½ + 24½ to 91½ + 24	1	4	Slight movement.
20	Nov. 7	42 + 32	4	3	
21	10	54 + 2	3-5	4	Two seen within the space of 3 seconds from apparently the same point.
22	10	87½ + 19 to 88 + 18	5	3	Slight movement.
23	16	69 + 15	5	4	
24	Dec. 12	117 + 11	> 1	3	
25	12	106 + 24	4	3	
26	12	117 + 24	4	4	Nearly stationary, from direction of *β Geminorum*.
27	12	112½ + 2 to 112½ + 1	3	3	Path 1°
28	1880, Jan. 2	102 + 10½ to 102 + 9½	4	3	Path 1°
29	7	98 + 3	4	3	Path ½°
30	Feb. 8	148 + 22	5	4	
31	April 30	236 + 19 to 235 + 19	2	2	Path 1°
32	May 8	257 + 1	4	3	Slight movement.
33	11	181 − 8	4	3	Visible 1 sec.
34	June 29	316 − 11	4	4	Visible 1 sec. accurate.
35	July 30	10½ + 50 to 10½ + 51	2	4	Path 1°
36	Aug. 8	102 + 58	1	3	Slow; 1 sec.
37	9	21 + 39	4	4	
38	9	44⅝ + 56¼	1	4	Visible 2 sec.
39	9	55 + 57	1	4	Visible 1 sec.
40	26	348 + 16½	3	4	Slight movement.
41	Sept. 29	6 + 5½ to 6 + 4½	5	3	Path 1°
42	30	359 − 14 to 358½ − 15	2	2	Path 1°
43	Oct. 8	27 + 36	4	4	
44	25	22 − 18	2	2	
45	31	54 + 7	2	4	Slow, 1 sec.

Catalogue of Radiant Points.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
1	1880, Jan. 5–10	140° + 7°	7	3	Meteors generally bright with medium length of path and velocity, and leaving persistent streaks. 5 seen during half an hour on the 7th. Confirms Tupman's position at 145° + 5°, Jan. 4–11, '70.
2	2	227 + 48	6	2	Meteors quite bright, long, and generally rather slow, all recorded from 7 to 8 hours.
3	Feb. 6–8	130 + 22	3	3	Meteors faint, short, and rapid.
4	8, 9	155 + 20	3	3	Meteors rather bright, short, and rapid. Schiaparelli and Zezioli at 153° + 21°. February 3.
5	1879, April 12–26	206 – 8	5	4	Meteors quite bright, short and rather slow, position very accurately determined from a 2nd-mag. stationary meteor recorded on the 14th.
6	20–26	242 + 24	5	2	Meteors quite faint, short, and rapid. A feeble return of the Coronids.
7	20–22	245 + 50	3	3	Meteors quite faint, short, and rapid; position well deduced. Draconids No. I.
8	22–26	253 + 5	5	4	Meteors bright, short, and rapid: position well deduced from a stationary meteor = 1st mag. recorded on the 23rd.
9	19, 20	272 + 41½	4	4	Meteors quite bright, short, and rapid, with streaks: blue probably a branch of the Lyraids?
10	19–21	274 + 34	10	3	Meteors generally bright, short, and rapid, and of an orange colour. Lyraids, position quite well determined.
11	June 13–23	283 – 12	9	3	Meteors generally very brilliant, short, and of medium velocity, two = ½ in brightness and two = 1st mag.*: the brightest were of an orange colour.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
12	1879, June 13–24	307° + 5°	4	3	Meteors bright, short, and rapid. Aquilids?
	1880, June 29	304 + 3	5	3	Meteors rather faint, generally rapid, and of medium length.
	29	295 − 1	4	3	Meteors generally quite bright, short, and rapid, possibly two distinct showers in this region?
13	1879, July 13–24	307 − 5	4		Meteors bright, short, and rapid. Aquilids. Compare with No. 12.
14	17–20	318 + 22	6	4	Meteors generally very bright, short, and rapid; position well determined.
15	13–24	338 + 18	5	4	Meteors generally faint, short, and rapid, position accurately deduced. Pegasids II.
16	1880, July 28	341 − 10	4	3	Meteors quite bright, short, and of medium velocity. Denning, = 341° − 13° July 21–29.
17	28–31	330 − 6	20	4	Meteors very bright, and generally slow, position very accurately deduced. Max. 29–30. Aquariads.
18	28–30	328 − 15	8	4	Meteors generally rapid and very bright. Max. 28th, position accurately determined. Nos. 16, 17, and 18 possibly all one shower?
19	29–31	315 + 5	12	4	Meteors of medium brightness, short, and rapid; position accurately deduced. Max. 29. Active shower.
20	July 30, Aug. 1	294 + 13	8	3	Meteors of medium brightness, short, and rapid. Max. July 31. Confirms Heis at 294° + 13° July 26–31.
21	30 1	303 + 8	4	2	Meteors very bright and slow; not accurate. Heis July 14, August 31 = 305° + 7°.
22	29 7	356 + 25	6	2	Meteors of medium brightness, short, and rapid, not accurate.
23	29 1	331 + 53	5	3	Meteors generally bright, long, and rapid.

No.	Date.	Position. R.A.	Dec.	No. of ↓'s.	Weight.	Remarks.
		°	°			
24	1880, Aug. 8. 9	5	+75	6	3	Meteors very bright, short, and rapid.
25	1879, Aug. 10–12	5	+57	5	3	Meteors very bright, short, and rather slow.
	1880, Aug. 8, 9	5	+55	14	3	Meteors quite bright, of medium length and generally rapid with streaks. Active shower.
26	8, 9	44¾	+56¼	48	4	Meteors generally bright, short, and rapid with streaks; position very accurately determined from several short tracks near focus and one perfectly stationary meteor = 1st mag. and visible 2 seconds. Perseids. Horary No. on 8th 16↓s. 9th 28↓s. 8th = R.A. 38° + 56°.
	1879, Aug. 10–12	44½	+57	106	4	Meteors bright, short, and generally rapid with persistent streaks. Position very accurately determined from 20 paths exactly mapped. Horary No. 24 on 10th, 13 on 11th, and 5 on 12th. Very meagre display.
27	1880, Aug. 9	55	+57	6	4	Meteors bright, short, and rapid with streaks. Position accurately deduced from several short tracks and one nearly stationary meteor. Probably branch of Perseids?
28	8, 9	270	+69	4	4	Meteors bright, short, and slow, accurate. Heis, August 13–31 = 275° + 69°.
29	8, 9	332	+60	7	3	Meteors generally not very bright, short, and rapid; principally seen on the 8th.
30	1879, Aug. 11	38	+60	8	4	Meteors generally bright, short, and rapid; position very accurately deduced from 7 well-observed tracks, and one absolutely stationary of the 2nd mag. and visible 1 sec. Perseids.
31	10–12	17	+42	4	2	Meteors bright, short, and rapid; strongly suspected.
32	10–12	23	+37	4	3	Meteors rather bright, short, and rapid; position well determined.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
33	1879, Aug. 10–12	180° + 78°	6	2	Meteors faint, long, and rapid; strongly suspected.
34	7–12	344 + 42	6	4	Meteors rather light, short and rapid; position accurately deduced. Honorids.
35	7–12	358 + 55	7	3	Meteors very bright, short, and rapid; position quite well determined, possibly same as No. 25?
36	20–23	240 + 12	4	3	Meteors rather bright and short with a medium velocity.
37	20–23	258 + 20	7	3	Meteors faint, short, and rapid.
38	1880, Aug. 28, Sept. 4	352 + 27	7	3	Meteors quite bright, short, and slow.
39	28 2	348 + 17	3	4	Meteors bright, short, and rapid; accurate.
40	31 2	2 + 16	5	3	Meteors short, not very bright, and rapid.
	1879, Sept. 9–11	2 + 15	9	4	Meteors generally faint, short, and rapid: accurate, principally seen on the 11th. Pegasids III. Heis = 1° + 14° September 16-31.
41	15–20	10 + 35	10	4	Meteors faint and rapid, with medium length of path; 5 of the number seen on the 17th. Andromedes I.
42	7–17	346 + 45	8	3	Meteors generally faint and rapid, with medium length of path. Radiant point appeared to shift from 343° + 45° to 349° + 45°.
43	1880, Sept. 25, Oct. 5	8 + 22	7	2	Meteors short, quite bright, and very slow; rather diffuse.
44	29 9	24 + 17	10	4	Meteors generally bright, short, and quite slow; average length of path 7° and duration about 1 sec. Very close agreement with the radiant point of the comet of 1769 = 24½° + 17° September 28.
45	Oct. 1	16 + 50	4	2	Meteors short, faint, and rapid; strongly suspected.
46	2-5	15 + 40	5	3	Meteors not very bright, rather short, and slow.

No.	Date.	Position. R.A. Dec.	Nos. of ↓'s.	Weight.	Remarks.
		° °			
47	1880, Oct. 5–8	27 + 24	5	3	Meteors bright, generally short, and rapid.
48	6, 7	50 + 10	4	3	Meteors quite bright, of medium length, and slow.
49	1879, Oct. 14–19	23 + 20	4	4	Meteors rather bright, short, with medium velocity; position well determined; one nearly stationary meteor.
50	19	26 + 48	4	3	Meteors faint, short, and rapid.
51	19	44 + 18	4	3	Meteors rather bright, short, with medium velocity. Denning October 2–19, '77 = 47° + 16°.
52	14–19	44 + 35	5	4	Meteors rather bright, short, and rapid; position well deduced.
53	19	48 − 4	4	3	Meteors bright, short, and rather slow; one nearly stationary.
54	19, 20	59 + 14	5	3	Meteors rather bright, short, and rapid. Taurids I.
	1880, Oct. 31, Nov. 3	60 + 18	5	3	Meteors quite bright, of medium length, and rapid.
	1879, Nov. 7–10	60 + 19	6	3	Meteors very bright, and generally slow, with long paths, orange coloured.
55	Oct. 19, 20	59 − 13	4	3	Meteors very bright, slow, and short. Tupman, October 5–6, '69 = 54° − 14°.
56	14–20	73 − 53	8	2	Meteors generally bright, slow, and of medium length, not accurate.
57	19	84 − 26	4	4	Meteors rather bright, short, and rapid; well determined.
58	19	92 + 15	8	4	Meteors not very bright, short, and rapid, with streaks; position very accurately deduced. Orionids.
59	19	350 + 50	4	3	Meteors faint, short, and rapid.
60	1880, Oct. 24	26 + 13	6	4	Meteors of medium brightness and length, and quite slow; accurate.
61	25–31	3 + 42	7	3	Meteors short, faint, and rapid.

No.	Date.	Position. R.A. Dec.	No. of ↓'s.	Weight.	Remarks.
62	1880, Oct. 31, Nov. 2	54°+7°	6	3	Meteors very bright, short, and slow.
63	24 3	43+24	7	3	Meteors short, slow, and bright.
	1879, Nov. 7–10	47+24	7	4	Meteors quite bright and short, with medium velocity, position very accurately determined from several short tracks, and one nearly stationary.
64	1880, Oct. 29, Nov. 3	325+65	4	2	Meteors of medium length, bright, and slow, with streaks, orange; strongly suspected.
65	1879, Nov. 10	42+43	4	3	Meteors faint and rapid, with medium length of path: strongly suspected.
66	10	65+30	5	3	Meteors bright, short, and rather slow; orange.
67	15	151+22	7	4	Meteors of medium brightness, short, and rapid, with streaks accurate. Leonids.
	1880, Nov. 11, 12	148+22	12	3	Meteors quite bright, short, and rapid, with streaks; Leonids.
68	1879, Nov. 15	146+1	3	3	Meteors short, and rather bright, orange; strongly suspected.
69	1880, Nov. 21–28	81+23	5	3	Meteors rather bright, of medium length, and slow. Taurids II.
	1879, Dec. 7–12	82+23	5	3	Meteors rather bright, of medium length and velocity. Taurids II.
70	7–12	75+45	5	3	Meteors short and bright; and radiant point very near *α Aurigæ*; strongly suspected.
71	7–12	100+35	13	3	Meteors generally faint, long, and rapid; position well deduced; possibly a branch of the Geminids?
72	7–12	110+33	20	4	Meteors bright, very short, and rapid; position well determined. Geminids.

The Binary Star, β Delphini (=β 151). By S. W. Burnham, Esq.

As a wide pair, *β Delphini* has been known as a double star since the time of Sir William Herschel, who detected the well-known companion, then about 30″ distant. Sir John Herschel subsequently found a fainter star a little nearer, the distance of which is now 28″ (the other being 35″). The measures show that both are mere optical companions, and have no physical relation to the principal star, the change in their positions being due to the proper motion of the bright star. In 1873, with an aperture of only 6 inches, I discovered that the principal star was composed really of two stars, differing in brightness something more than one magnitude. The distance then made it too easy a pair to escape attention with a small aperture. It very soon appeared from observations that the angle was increasing, while the stars were gradually approaching each other. It became more and more difficult, until finally, in 1879, it was single, or practically so, with the 18½-inch refractor of the Dearborn Observatory. This year it has commenced to open again, and although of the last degree of difficulty, and only observable under the best atmospheric conditions, I have measured it on four nights, with results as accordant as could be expected under the circumstances. The separate observations are as follows:—

P = 139°·5	D = 0″·28	1880·592
129·4	0 22	·613
127·1	0·30	·627
129·0	0·24	·766
142·9	0·27	·782

These distances are decidedly too large, as would be expected in attempting to measure a distance so small in comparison with the thickness of the micrometer-wires. The distance was estimated as not exceeding 0″·2. On each occasion the close pair of δ *Equulei* was measured, and that pair, the distance of which is 0″·35, was certainly at least double the distance of *β Delphini*.

The following measures will show the relative position of the companion, each year since its discovery, from the measures of Baron Dembowski and myself:—

β	1873·60	P = 355° ±	D = 0″·7 ±	Est.
Δ	74·66	15·6	0·61	5 nights.
Δ	75·65	20·1	0·54	4 „
Δ	76·66	25·8	0·48	4 „
β	77·79	40·8	0·32	4 „
β	78·65	53·7	0·24	4 „
β	79·54	Elongation, if any, in 90°; but very doubtful. Very good night.		
β	79·57	No certain elongation. First-class night.		
β	80·68	133·6	0·26	5 nights.

The observations show conclusively that we have here one of the most rapid binary systems known, the angular motion in the last two years being not less than 80°. It is too early to speculate upon the probable period, but the measures of the next three or four years will go far to give at least an approximate determination. It is to be hoped that double-star observers will give this interesting system special attention. By next year it will probably be comparatively easy of measurement. An increase in the distance of one-tenth of a second will be sufficient for this.

Chicago,
1880, *December* 15.

Remarks on Dr. Hartwig's Recent Determination of the Lunar Physical Libration. By the Rev. C. Pritchard, Savilian Professor of Astronomy at Oxford.

It will be in the recollection of the Society, that in June last I announced the completion of the mathematical and computational parts of the investigation of the Lunar Physical Libration, as derived from 59 measures of Lunar Photographs, and that another similar set, made with reference to a second spot upon the Moon, was nearly finished. Meanwhile Dr. Hartwig has just published a memoir detailing the results of 42 measures, made at Strassburg with a Heliometer, for the attainment of the same object. That memoir, I do not doubt, deserves and will find an honourable place in the Annals of Astronomy, and he will himself meet with the sympathy of astronomers on the successful termination of his labours in this behalf.

I find that the results obtained by Dr. Hartwig are in substantial, though not absolute, agreement with my own. An absolute agreement was scarcely to be expected, owing, not to the difficulty and to the length of the various processes employed, but to the fact that the principal quantity ultimately sought for could not considerably exceed a second of arc when seen from the Earth; and, independently of this fact, any inevitable errors in certain parts of the processes must entail 250 times the same amount of error in the ultimate result.

I will now so far anticipate the communication which I hope speedily to lay before the Society, as to place in juxtaposition some of the principal results of the researches in which we, and a few other astronomers, have been engaged.

The first of these researches was that published by Bouvard, at Paris, in 1822; the second by Kreil, at Milan, in 1837; the third by Wichmann with the Königsberg Heliometer in 1848; the fourth by Hartwig, with a Heliometer at Strassburg in 1880; and, finally, that by myself at Oxford, with photographs taken

in the focus of the De la Rue Reflecting Telescope, of which notice was given in the summer of 1880.

First, as to the mean Inclination (I) of the Lunar Equator to the Ecliptic; secondly, as to the maximum amount of the Lunar Physical Libration in Selenographic Longitude (L).

	I.	L.
	° ′ ″	″
Paris Determination	1 28 45	289·7
Milan	1 35 48	342·6
Königsberg	1 32 9	389·
Strassburg	1 36 34	207·
Oxford	1 32 58	350·

With regard to the last three, I do not think that, taking all the elements of the case into consideration, there exists ground for expecting a closer agreement, the reasons for which opinion will be stated in the sequel.

It is now necessary to state that the entire foundation of the researches hitherto undertaken, has been the measurement of the distances of some well-defined spot on the Moon, from various points in its periphery; and, in general, seven or eight such measures have been taken for a single observation. Wichmann and Dr. Hartwig selected the same spot, Mösting A. At Oxford, two spots have been selected, one of which, Triesnecker B, has the advantage of being much nearer to the Moon's mean centre than is Mösting A. In the course of the investigation the Selenographical Longitudes and Latitudes of these spots become necessarily determined. The results obtained by Wichmann and Dr. Hartwig are not in absolute accordance. They are as follows:—

	Wichmann.	Hartwig.
	° ′ ″	° ′ ″
Selenographic Longitude of Mösting A . . .	5 13 23	5 10 57
Selenographic Latitude	3 10 55	3 10 21

These coordinates imply a discordance of nearly three minutes of a great circle, as seen from the Moon's centre, but as seen from the Earth they indicate an uncertainty of a little under a second of arc. It may here be proper to observe that notwithstanding these different determinations of the coordinates of the same spot by two highly accomplished and successful observers, each employing upwards of forty distinct determinations, Dr. Hartwig comes to the conclusion that the probable error of a single determination of place, as seen from the Earth, is only ±0″·43. I think it becomes a question whether the discordance may possibly and properly be assigned to the use of instruments having different apertures. The fact exists. Does this also tend to explain the discordances in the determination of the inclination of the Moon's Equator to the Ecliptic?

It is also desirable to remark on Dr. Hartwig's method of avoiding errors arising from any irregularities in the contour of the Moon's limb. He takes seven places on the limb, distributed with considerable uniformity; but, in making a measure of the distance of the spot with the Heliometer, wherever his eye could perceive the necessity for it, he has regard to what appears in his judgment to be the average level of the contour.* I have myself adopted a different method, which is this: inasmuch as the photograph lies quietly and undisturbed under the microscope of the De la Rue Micrometer, an opportunity is given of leisurely selecting those places where little or no serrature exists. I may add also that, in the case of photography, there are no changes of the Moon's diameter during the time of observation. Changes of this nature necessarily introduce considerable trouble in the heliometer determinations.

While on this subject of the irregularities of the Moon's limb it is proper to refer to what Dr. Hartwig states as the result of his own experience. He altogether and decidedly declines to accept what he terms the emphatically expressed conviction of Mr. Neison as regards the introduction of serious periodical errors from this source. My own long experience is in entire accordance with the opinions of Dr. Hartwig.†

I may also add that after listening to Mr. Neison's strictures on this behalf, I instituted a series of measures round the entire visible periphery of a Lunar photograph, avoiding no irregularities whatever; and the result was such as convinced me that the irregularities themselves, in this particular case, did not interfere with the exactness of computed results. But there was another conclusion which was unavoidably impressed on my own mind, from these and similar researches. Dr. Hartwig, in the course of his admirable Memoir, refers to the uncertainty of the determination of the Selenocentric Longitudes when compared with those of the Selenocentric Latitudes. This comparative insecurity which has been pointed out by Wichmann, had been sensibly observed by myself. Experiments at last indicated its probable cause; and, towards the conclusion of the work, I discovered that the source of error might be very much avoided by increasing the number of measures, and by distributing them in particular and ascertainable directions. The true cause originates, I believe, in the necessary want of symmetry of the Moon's illuminated periphery, with respect to the selected spot on the Moon's disk. The configuration of the spot, relative to the illuminated periphery, is always unavoidably one-sided in Longitude. Owing to this cause, Dr. Hartwig, and Wichmann before him, considered themselves justified in applying certain weights (more or less arbitrarily determined) to the Latitude equations of condition. I have myself hesitated, and am still

* Cf. p. 27, Dr. Hartwig's *Memoir*.
† Page 31, *Op. cit.*

inclined to hesitate, to adopt this process; but I fear I must now reluctantly acquiesce in this method, in order to place the results in fair juxtaposition. Should I undertake, as I propose, to complete another set of 100 measures, I intend to take ten individual measures, properly selected, for each determination; and then I shall decline to weight the Selenocentric Latitude equations.

The general conclusions which in my judgment are arrived at by a comparison of the Strassburg with the Oxford researches are these:—

1. There does exist a real, but inconsiderable, libration in Longitude, less than five minutes of arc, and probably not ascertainable within some decades of seconds.

2. The Moon is sensibly spherical.

3. There appears but little hope of ascertaining the position of a spot upon the Moon with sufficient accuracy to admit of its use, in preference to the limb, in astronomical observations of the Moon's place.

In conclusion, I beg to tender Dr. Hartwig my hearty congratulations on the successful termination of a research, of the difficulties and delicacy of which I have a painful familiarity.

Oxford,
1881, *Jan.* 13.

Description of an Observing Seat for an Equatoreal. By Professor G. W. Hough, Director of the Dearborn Observatory.

In making micrometer measures with the equatoreal telescope, it is desirable to have a convenient seat for the observer. A person in a cramped or unnatural position can hardly do as good work as when the body is resting in an easy and natural manner.

The problem of a perfect seat for the Equatoreal can hardly be said to have been solved, although a number of elaborate and costly chairs have been constructed by different observers. I presume the majority of those observing with a large Equatoreal use a simple step-ladder, provided with various cushions, &c., for securing the proper elevation of the eye. This simple apparatus seems to be the most expeditious for actual work, notwithstanding the trouble of always getting in just the right position.

The Dearborn Equatoreal was provided with an elaborate and costly chair, constructed after the model of that devised by Bond, for the Harvard University telescope.

This piece of mechanism was found to be so unwieldy, and

required so much expenditure of muscle to bring the seat in position, and moreover consumed so much time for its manipulation, that practically it has never been of any use. Nearly all observations have, accordingly, been made by the use of an ordinary step-ladder.

During the past year Mr. S. W. Burnham and myself

decided that an adjustable seat attached to the step-ladder would add greatly to the convenience of the observer, and would also economise the time in preparing for micrometer measures. A light and easily manipulated chair is especially desirable for double-star work, since many different objects are often examined on the same night.

After considering the problem, the following mechanism was devised, which, after some months of constant use, seems to answer the purpose admirably.

The accompanying drawing will clearly show the seat mechanism.

Over the central portion of a step-ladder 11 feet in height and 5 feet in width, are screwed two strips of pine, S, S, 2 inches in width, which pass from the top to the bottom of the steps; the distance between them being about 7 inches.

A light wooden frame is made to slide on this track, to which the observing seat, C, is attached. The seat is 20 inches in length and 12 inches in depth. In order to keep it in place, the tracks S S are bevelled on the inside, and the frame is made with a corresponding bevel like an ordinary slide-rest.

The seat, when in use, is raised and lowered by means of the following device.

To the top of the steps is attached a woven rope, three-eighths of an inch in diameter, which is passed *once* around a fixed drum, A, 5 inches in diameter, attached to the seat-frame, thence over a loose pulley, B, thence back again to the top of the steps over a pulley, where a weight, W, is attached just sufficient to balance the seat. The weight W slides in vertical tracks attached to the rear of the step-ladder.

The seat, therefore, is held in position simply by the *friction* of the cord around the fixed drum A, without the use of any clamp or stop.

The whole weight of the seat and frame is about 15 lbs. A very slight force, viz. 10 lbs., will cause it to slide up the track when unloaded.

When seated on the chair, a slight pull downwards, on the rope to which the counterpoise is attached, causes it to descend any amount desired. As the whole merit of the apparatus consists in the ease and celerity with which the observer may secure the proper elevation for the eye, it may be added that the seat is completely under control; it may be moved an inch, or a foot, almost instantaneously, and in this respect is infinitely superior to any screw or rack motion.

For the upward movement it is necessary to rest one's weight on the steps.

As there are no ratchets or stops, but simply a rope wrapped round a smooth drum, the chair can be manipulated in the dark without difficulty. The cost of applying this apparatus to the step-ladder was about five dollars.

When using the micrometer, a small table, D, for holding the note-book, lamp, eye-piece, &c., is attached on the right-hand side of the seat, and may be removed at pleasure. The step-ladder itself is mounted on castors and is easily shifted to any required position.

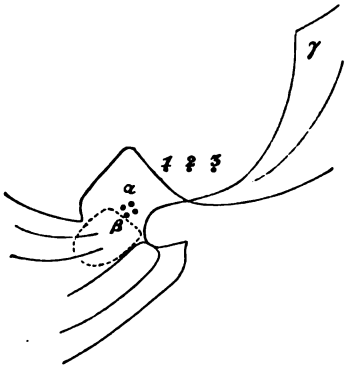
When micrometer measurements are not in progress, the seat may be slid to the top of the steps, where it is out of the way, and the ladder used in the ordinary way.

A step-ladder built to conform to the radius of the telescope and provided with a sliding seat similar to the one described, could be mounted on tracks so as always to be at the right distance from the telescope, but we doubt whether it would be as convenient as one free to move over the floor in any direction.

Chicago, U.S.A.
1880, *December*.

Note on the comparative Brightness of the Nebula of Orion.
By E. B. Knobel, Esq.

At the January meeting of this Society a photograph of the Nebula of *Orion*, by Dr. H. Draper, was exhibited, and remarks were made upon it which appear to require a slight correction. It was stated by Mr. Ranyard that, "comparing the photograph with the drawings of Bond, Secchi, Liapounov, and others, it corresponds with none of them. The most curious difference is



the little bright nucleus preceding the trapezium, which is the brightest part in the photograph, but that is not the case in *any* of the drawings." And further the remark was made that "Dr. Draper's photograph shows that a nebulous mass on the preceding side of the trapezium is the brightest region of the nebula. This does not correspond with any of the drawings."

In the engraving of Bond's drawing which I possess, the brightest portion of the nebula appears certainly to be the mass on the preceding side of the trapezium; and in one of the most recent and carefully executed drawings of this nebula, by M. Trouvelot, made with the 26-inch Equatoreal, and published in the "Description of the Instruments of the United States Naval Observatory, Washington, 1876," the portion of the nebula preceding the trapezium is shown very considerably brighter than

any other part of it, confirming Bond's drawing and Dr. Draper's photograph.

I have endeavoured to make a few photometric observations for the purpose of determining the brightest portion of this nebula. With that object I have employed an 8½-inch silvered glass reflector with an *unsilvered* glass flat. The light is thus very considerably reduced; and then by applying the limiting-aperture astrometer, described in *Monthly Notices*, vol. xxxv. p. 100, I am able to extinguish successively every portion of the nebula, and by this means obtain an approximation to the light-ratio of its several masses.

Bad weather has prevented my securing very many results, as it is so necessary that such observations should not be undertaken except on very fine, clear nights. The power used was 120.

1881, Feb. 28. Fine clear night. Seeing good from 7 to 9 P.M.

Aperture 8½ inches. The portion of the nebula of *Orion* surrounding the ray marked γ in the diagram is faintly visible.

Aperture 4·92 inches. The ray γ extinct. The limiting aperture for γ was not determined, but the ray could not now be glimpsed.

The only nebula visible consists of the central portions marked α and β. The portion β extends north preceding the trapezium rather more than the distance from Star 1 to the trapezium, and at an average position angle of 340° or 350°, as roughly indicated by the dotted line in the woodcut.

Aperture 3·37 inches. Nebula faintly visible to nearly the same extent as in the preceding observation. Portion α much fainter than β.

Aperture 3·01 inches. Star 3 faintly visible.

Aperture 2·52 inches. Star 3 extinct. Portion α of nebula seen by averted vision. The only nebula visible directly, stretches from the trapezium in a north preceding direction.

Aperture 2·10 inches. Portion α of nebula extinct. A slight haze visible north preceding the trapezium.

Aperture 1·67 inches. All nebula extinct by direct vision, a very slight haze glimpsed by averted vision at the spot marked by the letter β.

Aperture 1·33 inches. Not a trace of any nebula or haze.

March 8, 7 P.M. to 8.30. Fine and clear, no haze. Moon 8 days old.

Apertures 8½, 4·92, and 3·37 inches confirm the observations of Feb. 28.

Aperture 2·65 inches. Portion α of nebula is glimpsed by averted vision. The only nebula visible looking direct is the part marked β.

Aperture 1·82 inches. Portion α of nebula extinct. β faint.

Aperture 1·46 inches. Star 3 extinct; only two stars seen in trapezium. Slight haze on north preceding side of trapezium.

Aperture 0·91 inches. Nebula quite extinct; only one star distinctly seen in trapezium. Star 2 seen with difficulty.

For comparison, the objects not differing much in altitude, I made the following observations of the *Andromeda* nebula, power used being 80:—

March 8.

Aperture 8½ inches. The nucleus of the nebula is round and bright, like Trouvelot's drawing in the Harvard College Series.

Aperture 2·46 inches. The only nebula visible is a small round luminous spot, the diameter of which is about one-half that seen with 8½ inches aperture.

Aperture 1·95 inches. Nebula absolutely extinct. No glimpse of it by any means.

The mass α disappears uniformly, without apparently diminishing in extent; but the dotted portion, marked β, becomes smaller as the light is reduced, the brightest part being at the position marked in the diagram by the letter β.

In the above observations, it should be noted that the actual apertures which would extinguish the portions of the nebulæ referred to, are probably greater than those I have given, my object being to proceed by wide steps so as to be able to say with certainty that with one aperture the object was visible, and with the second aperture the object was not visible. It would have required many more observations than it was possible for me to make, to determine the exact aperture when each feature became extinct.

If we assume for the purpose of a rough approximation, that 1·8 in. and 1·3 in. are the limiting-apertures respectively of portions α and β of the nebula of *Orion*, then these will give a light ratio of 0·5216. And comparing the nebula of *Orion* with that in *Andromeda* in the same way, we have rough limiting-apertures of 1·33 in. for the former, and 1·95 in. for the latter, which give a light-ratio of 0·4652.

Though these figures must be received with caution, we may, I think, place more confidence in the proof that the central portion of the nebula of *Orion* preceding the trapezium is considerably brighter than that on the following side; and that the nebula in *Andromeda*, though so conspicuous to the naked eye, is decidedly less bright than the central portion of that in *Orion*.

I venture to think that this class of observation affords a far

more reliable method for determining any change in a nebula than the comparison of drawings or photographs, at any rate until the art of celestial photography is very much improved.

1881, *March* 11.

Further Notes on the Nebula in the Pleiades. By Maxwell Hall, Esq.

My first opportunity for observing the Nebula in the *Pleiades* since my last visit to England occurred on the night of the 21st November this year; meteorological work had detained me in Kingston.

As soon as the telescope was turned to *Merope*, a change in the preceding boundary of the Nebula was noticed; it was found to extend as far as *Electra*, and the parabolic form of the Nebula, as seen in 1877, was destroyed. (*Monthly Notices*, vol. xl. p. 89.)*

The new form of the Nebula was carefully drawn that night, and a copy of this drawing is sent herewith, together with the former drawing of 1877 for the sake of comparison.† Observations made on other nights confirmed the new boundary.

On the night of Dec. 2 the *Pleiades* were carefully examined with reference to the drawing made by Mr. Common at Ealing (*Monthly Notices*, vol. xl. p. 376); but I failed to make out any of its features.

The Supplementary Number of the *Monthly Notices* reached me a few days ago, and I was greatly pleased with Prof. Tempel's beautiful drawing of the *Pleiades* and the *Merope* Nebula (vol. xl. p. 622); his remarks are also very valuable.

Now, it will be seen that the form of the Nebula, as drawn by Prof. Tempel, is exactly the mean of the two forms drawn by me in 1877 and in 1880; and although Prof. Tempel says that he has never observed any change in the Nebula, yet I cannot refrain from asking him in these pages whether there is not, at the present time, an extension of the Nebula in the direction of *Electra*. As I remarked before, the Nebula has been seen to extend beyond *Electra* as far as *Celæno*; and I am anxious that the present indications of systematic change should be confirmed by Prof. Tempel and other observers.

Dr. Wolf has also favoured us (*Monthly Notices*, vol. xl. p. 293) by calling attention to his description and chart of the *Pleiades* published in the *Annales de l'Observatoire de Paris*; but unfortunately the volume in question was not in the library of the Royal Astronomical Society during my visit to England, and I have to gather his views from the few remarks made in the

* The drawing was published in the next No. of the *Monthly Notices*, p. 169.

† The new drawing shows an extension from the Nebula from above *Merope* (23) to *Electra* (17), and then nearly N. to S., uniting itself with the parabolic figure.—Ed.

Monthly Notices. The appearance of nebulosity throughout the whole group, excepting small circular areas concentric with the brighter stars, is indeed very striking; but on this subject we shall do well to await the further communications of Prof. Tempel.

Jamaica,
1880, *December* 13.

The Nebulæ in the Pleiades. By Henry Pratt.

After comparing the great differences of representation of these objects as seen by Maxwell Hall's 4-inch refractor and Mr. Common's 37-inch reflector, I was about to send a copy of my drawing made in October 1876, as giving the more usual appearance of the large Nebula (Tempel's) in a telescope of moderate aperture, when happily Signor Tempel's beautiful map of the whole group of the *Pleiades* appeared. Therein I find the most perfect representation of the Nebula as shown by my 8·15-inch With-Browning speculum, with one slight exception. I always see the Nebula extend to some little distance beyond *Electra* in a fainter condition than the portion drawn by Tempel, the latter corresponding to the brighter part of my drawing. As several observers appear to be still unsuccessful in their search for this object, I wish to say that during the last few years I have never looked at the *Pleiades* without also seeing the Nebula. Possibly a suitable power has something to do in the matter. I see it well with 45, Kellner, giving a field of 85′. but I prefer the view rendered by 84, Kellner, with a field of 50′. The light of the brighter stars near is quite sufficient to obliterate every trace of the small detached Nebula (Common's) between *Alcyone* and *Merope*. Indeed, as Signor Tempel with a larger telescope than mine, and an Italian sky, appears to question its existence, it may be doubted whether it is not impossible for so small a telescope as mine to show it at all. But I wish to say a word in favour of our foggy land, for in practice I find a very simple contrivance sufficient to render the existence of this small Nebula apparent even in my telescope. By applying a slotted diaphragm to power 84, so arranged as to exclude *Merope* and *Alcyone* and to show nearly the whole space between them, I am able to see just a trace of nebulosity where Mr. Common has drawn it. It is so faint, and fades so gently, that I cannot well define its boundary, but the major axis of its brightest part lies in about the direction given on Mr. Common's drawing, and I am able to see decidedly the deeper blackness of the sky between this Nebula and *Merope*. The faint stars mentioned by Mr. Common and Signor Tempel, and suggested by the latter to be the illusory cause of the nebulous appearance, I also distinctly see.

18 *Preston Street, Brighton*,
1881, *January* 11.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLI. APRIL 8, 1881. No. 6.

E. J. STONE, Esq., Vice-President, in the Chair.

Boyd Moss, Esq., Clatford, Middlesex,

was balloted for and duly elected a Fellow of the Society.

On the Solar Parallax, derived from Observations of Mars at Ascension, in 1877. By David Gill, H.M. Astronomer at the Cape of Good Hope.

(*Abstract.*)*

Aided by a grant of 250*l.* from the Society, a like sum from the Government Grant Fund of the Royal Society, and with the loan of Lord Lindsay's Heliometer, Mr. Gill undertook an expedition to the Island of Ascension for the purpose of determining the parallax of the Sun, from observations of the diurnal parallax of *Mars*, during the favourable opposition of that planet in the year 1877.

After a brief history of the expedition, Mr. Gill describes the methods and instruments used, and gives the results of the operations by which the latitudes and longitudes of the observing stations were determined.

The stars used for comparison with *Mars* were observed in the meridian at thirteen of the principal Observatories. The result of a discussion of all these observations is given, and the proper motions are deduced from a discussion of all the old observations. A series of very accurate *preliminary* places is thus formed for comparison with the results of the heliometric

* The Memoir is being printed, and will appear in the next volume of the *Memoirs*.—ED.

triangulation. In the latter operation a night's work was so arranged that first a *standard distance* was measured, next three other distances in the triangulation, and then again the *standard distance.*

This *standard distance* was taken to be the angle between the stars $e-g$ for 1877·0. An approximate value of this angle (7054″·80) was adopted, the true value being assumed

$$7054''.80 + \Delta S.$$

From each measure of this *standard distance* the scale value was deduced in terms of ΔS, and from the different values so found the scale value was interpolated for the epoch of each observation. Thus in all the observations of the triangulation the deduced distances were affected systematically only by

$$\Delta S \times \frac{\text{measured distance}}{\text{standard distance}}$$

and by such errors as might exist in the heliometer by which the scale value would be different for the measurement of large and small angles. For example, if the radius of the cylindrical slides in which the segments move is different from the focal length of the object-glass, an error depending on the square of the distance would be produced.

Accordingly, each measured distance is supposed to be affected by a term d depending on the square of the distance, and by a term

$$= \Delta S \times \frac{\text{measured distance}}{\text{standard distance}}.$$

The tabular Right Ascensions of the preliminary star-places are supposed to be affected by errors that can be expressed by

$$ma + m^2b,$$

where m=the difference in magnitude of the star from 4·5 mag. (so that for 6th mag. $m=+1{\cdot}5$). The proper factors of these terms are formed for each comparison of the tabular distances (computed from the *preliminary places*) with the heliometer measures of the same distance.

In this way 70 equations are formed, involving the four unknown quantities

$$a,\ b,\ d,\ \text{and}\ \Delta S.$$

Approximate assumptions are made as to the mean error of the preliminary Catalogue and that of the heliometer measure—the latter being derived from measures of 19 pairs of stars which have been observed more than once. The *mean error* of a heliometer measure was $\pm 0''{\cdot}22$ corresponding with a probable error of $\pm 0''{\cdot}15$.

A system of weights was adopted such that the mean error of an equation of weight unity should be $\pm 0''\cdot 50$, and the solution showed a mean error of $\pm 0''\cdot 55$, so that the weights originally adopted were considered sufficiently accurate.

The average weight of an equation was 2·3. The solution of these 70 equations by least squares, having regard to the weight of each equation, leads to four normal equations, and a discussion of these shows that the terms b and d have no reality, since their omission from the equations makes no sensible increase of the sum of the squares of the residuals; but the omission of the term a increases the sum of the squares of the residuals from 23·0 to 36·8. When allowance is made for the errors of the heliometer measures, it is shown that the square of the mean discordance between the heliometer and meridian observations is reduced as $2\frac{1}{2}$ to 1 by the introduction of the correction depending on magnitude. The value of the term so determined is

$$a = -0''\cdot 27 \pm 0''\cdot 07,$$

or, in other words, *the Right Ascensions of the preliminary Catalogue have to be diminished fully quarter of a second of arc per magnitude fainter than* 4·5.

Applying this correction to the preliminary Right Ascensions, the R.A.'s of each Observatory are separately compared with the corrected Catalogue so formed, each comparison giving an equation of the form

$$a + mb + m^2 d = \mathrm{C} - \mathrm{O}.$$

Normal equations are then formed, and from these the values of a, b, and d are found for each Observatory.

Applying the corrections thus deduced, the whole of the observations are rediscussed and a definitive Catalogue is formed, corresponding systematically with the heliometer observations.

[The remarkable result comes out, viz. that the eye and ear observations correspond, within the limits of probable error, with the results of the heliometer measures; but the chronographic observations require large corrections depending on magnitude.]

With the star-places so found, and with Le Verrier's Tables of *Mars*, corrected for an assumed constant error of $+2''\cdot 45$ in heliocentric longitudes (derived from the Greenwich Meridian Observations), and employing $8''\cdot 80$ for the Mean Hor. Solar Parallax and $\frac{1}{300}$ for compression of the Earth, the apparent tabular distances of *Mars* from the comparison stars are computed and compared with the heliometer measures. From this comparison equations are formed on the type

$$f'\Delta\alpha + f''\Delta\delta + f'''n + \nu z = \mathrm{O} - \mathrm{C} + rm - f'(\tau - \tau_0)\kappa + f''(\tau - \tau_0)\kappa';$$

where

f' and f'' are the factors in R.A. and Decl.

n, the unknown percentage of the error of the assumed parallax.

f''', the effect on the distance of increasing the parallax one per cent.

ν, $\frac{\text{The observed distance in seconds of arc}}{10000}$.

τ, the Greenwich mean time of observation expressed in days.

τ_0, the mean epoch of the combination to which each observation in that combination is reduced.

κ and κ', the daily rate of increase of the real over the tabular R.A. and Decl. at the epoch of observation.

These equations are then formed into combinations as follows:—

1. Each combination must consist of two or more *bunches* of observations. Those *bunches* must be symmetrical evening and morning, and must afford data for eliminating at least the scale value and parallax from their combination. *Bunches* are only to be considered corresponding and symmetrical when the comparison stars have been the same and have been observed in the same way in the evening and morning.

2. The mean epoch of each *bunch* must be within twenty-four hours of the mean epoch (τ_0) of the combination, since κ and κ' may legitimately be considered uniform during that period.

3. The observations within each *bunch* must be made without much interruption by cloud, &c., so that the scale value may be considered uniform throughout each *bunch*.

Subject to these conditions it was found that twenty-five combinations could be formed from the observations, but of these three, from unfavourable conditions, could not be included in the definitive result, though their introduction produces no sensible change in the resulting parallax or its probable error.

The following are the steps of reduction:—

1. Normal equations are formed for each *bunch*.

2. z is eliminated from the normals for each bunch, preserving the symmetry of the equations.

3. The normals of the same name in each combination are added together and so normals in $\Delta\alpha$, $\Delta\delta$, and n are found for each combination. From these $\Delta\alpha$ and $\Delta\delta$ are then eliminated, and an equation in n, with the coefficient of the true weight of n, is obtained for each combination, in terms of κ and κ'.

4. Neglecting κ and κ' an approximate value of n ($=-0{\cdot}25$) is obtained, and when this is substituted in the normals $\Delta\alpha$ and $\Delta\delta$, the values of $\Delta\alpha$ and $\Delta\delta$ are obtained for the epoch of each combination.

5. It is then found that the separate determinations of Declination may be represented by

$$\Delta\delta = -0''.66 + .0059\, t + 0.00077\, t^2,$$

(where t is the time, in days, from September $5^d.5$) with a probable error of only

$$\pm 0''.057$$

for each determination of declination.

But the Right Ascensions cannot be represented so accurately. There is a strangely marked tendency in the run of the values of $\Delta\alpha$ indicating a variation having a period of about 13·6 days. Some of the observations cannot be represented within reasonable limits without the supposition of such a curve, but the general run of the observations is expressed by

$$\Delta\alpha = +0''.44 - .0114\, t - .00112\, t^2 \qquad (1)$$

But *all* the observations are well represented by the expression

$$\left.\begin{aligned} \Delta\alpha = {} & +0''.43 && \pm 0''.07 \\ & + .0035\, t && \pm .0032 \\ & - .000367\, t^2 && \pm .00016 \\ & + \begin{pmatrix} 0.376 \\ \pm .071 \end{pmatrix} \times \sin\,(u + 122°.3 \pm 12°.6) \\ \text{where } u = {} & \frac{t}{13^d.626 \pm .067} \times 360 \end{aligned}\right\} (2)$$

and the probable error of each determination of $\Delta\alpha$ is $\pm 0''.199$.

[The coincidence between this period ($13^d.63$) and that of a term depending on twice the Moon's mean longitude is remarkable, but the author can find no grounds for such an inequality, nor does he consider the real existence of such a period to be as yet proved.]

6. The values of κ and κ' are then found for the epoch of each combination by the above expressions for $\Delta\alpha$ and $\Delta\delta$.

When κ is found by (1)

$$n = -.209, \quad \text{or } \pi = 8''.782 \pm 0''.0116.$$

When κ is found by (2)

$$n = -.251, \quad \text{or } \pi = 8''.778 \pm 0''.0119.$$

The probable errors of a single measure of distance are by (1)

and (2) respectively $\pm''\cdot234$ and $\pm''\cdot241$, or about double the probable error of measures of stars and minor planets.

The author then examines the various sources of systematic error.

It is shown, by terms carried throughout the discussion, that if *no corrections for refraction* whatever had been employed, the resulting parallax would have been increased by $+0''\cdot014$, and as Bessel's refraction tables cannot be supposed $\frac{1}{100}$ part in error, no conceivable error in the refractions can produce an error of $0''\cdot00014$ in the deduced parallax. This is due to the symmetrical arrrngement of the comparison stars, and it is shown by an independent discussion, that even when the error of scale value is not eliminated from every bunch, as in the definitive reduction, such is the general symmetry of arrangement of the comparison stars that the value of the parallax comes out the same, and with practically the same probable error.

Mr. Gill comes to the conclusion, after an examination of every source of systematic error, that the only systematic error to which the result can be supposed liable, may be a small error due to chromatic dispersion of the atmosphere, but he also shows that this is probably very small from the close agreement between the resulting parallaxes at great and lesser zenith distances.

The following are the separate results:—

When only symmetrical combinations are employed (definitive discussion):

From symmetrical observations combined ...	$8''\cdot777 \pm 0''\cdot013$
Rejecting 3 combinations observed under unsatisfactory circumstances... ...	$8''\cdot782 \pm 0''\cdot012$
Rejecting 3 combinations on the supposition of a periodic variation of the R.A.	$8''\cdot778 \pm 0''\cdot012$

When the scale value is supposed constant, its error not being eliminated as in the previous discussion:

From all symmetrical observations combined ...	$8''\cdot778 \pm 0''\cdot014$
Rejecting 3 doubtful combinations ...	$8''\cdot784 \pm 0''\cdot013$

When the whole of the observations are combined, and the star-places are regarded as absolutely known:

From all the observations combined	$8''\cdot783 \pm 0''\cdot026$
Observations of greater zenith distance.	$8''\cdot786 \pm \cdot034$
„ lesser zenith distance...	$8''\cdot780 \pm \cdot040$
„ before opposition	$8''\cdot791 \pm \cdot034$
„ after opposition ...	$8''\cdot772 \pm \cdot040$

The separate results of the definitive discussion are:—

Date.	π	Dif. from Mean.	Weight of n.
1877, July 31 E and M	8″·90	+0″·12	·113
Aug. 5 E and M	·65	− ·13	·086
12 E, Aug. 10 M	·80	+ ·02	·175
14 E, Aug. 13 and 14 M	·83	+ ·05	·180
15 E and M	·80	+ ·02	·079
16 E and M	·90	+ ·12	·091
19 E and M	·88	+ ·10	·109
24 E and M	·77	− ·01	·227
29 E and M	·84	+ ·06	·113
Sept. 4, 5, 6, E and M	·82	+ ·04	·547
6 E and M	·67	− ·11	·057
6 E and M	·62	− ·16	·097
7 E and M	·83	+ ·05	·060
8 E and M	·81	+ ·03	·134
8 E and M	·70	− ·08	·097
9 E and M	·83	+ ·05	·145
10 E, Sept. 9 E and M	·65	− ·13	·309
10 E 10 M	·66	− ·12	·106
19 E and M	·76	− ·02	·077
24 E and M	·71	− ·07	·109
27 E Sept. 25 M	·86	+ ·08	·147
Oct. 3 E and M	·81	+ ·03	·110

The definitive result of the whole discussion gives

$$\pi = 8''{\cdot}78 \pm 0''{\cdot}012,$$

which corresponds with 93,080,000 miles for the mean distance of the Earth from the Sun.

This value of π is compared with that from the recent accurate determinations of the velocity of light by Cornu and Michelson; when this velocity is combined, on the one hand, with the constant of aberration, and, on the other, with the time occupied by light in traversing a mean radius of the Earth's orbit as derived from observations of *Jupiter's* satellites.

Michelson's determination of the velocity of light combined with Struve's constant of aberration gives

$$\pi = 8''{\cdot}81.$$

Cornu's determination combined with the same constant gives

$$\pi = 8''{\cdot}80.$$

But the mean of all the best modern determinations of the constant of aberration gives, instead of Struve's value (20″·445), the somewhat larger value 20″·496, and Nyrén's discussion (*Mem. St. Petersburg Acad.* vol. xix.) of the best modern Pulkowa determinations (including Struve's) gives 20″·481.

Maclear's very accurate observations of the Declination of α and β *Centauri* at the Cape give 20″·53 and 20″·59 respectively for the constant of aberration, showing, so far as their evidence goes, that the constant of aberration is not less when determined from Southern than from Northern stars.

If, as other results would appear to show, Struve's constant of aberration should be increased by 0″·05, then the resulting solar parallax from the combination with this constant would be diminished by 0″·02, and the values of π would be

$$\begin{aligned} &\text{According to Cornu} && 8''\cdot78, \\ &\text{Michelson} && 8\cdot79, \end{aligned}$$

values practically identical with the results derived from the heliometer observations of *Mars*.

Glasenapp's determination of the light equation (k) derived by him from eclipses of *Jupiter's* satellites gives

$$k = 500\cdot8 \pm 1\cdot02,$$

and this combined with Michelson's and Cornu's velocity of light gives

$$\pi = 8''\cdot76.$$

But if Glasenapp's determination of k is diminished in the direction of Delambre's value by the amount of its probable error—viz. 1ˢ—then the resulting parallax becomes

$$\pi = 8''\cdot78.$$

The large values of π which have been determined from meridian observations of *Mars* are explained by the fact that, at the altitudes at which the planet was observed in Europe in 1877, the limbs of *Mars* are spread by chromatic dispersion of the atmosphere into a fringe of coloured light 2″·2 broad. For the upper limb the violet part of the fringe is projected outside the disk of the planet, and for the lower limb the red part of the fringe is outside. In the field of a meridian instrument, illuminated by white light, more of the violet than of the red false limb would be obliterated, and hence the observer will cut more deeply with the spider-web into the false violet than into the more glaring red limb. Thus all the zenith distances measured would be too great, and the resulting parallax would also be too great. The results of other methods of determining the parallax

are considered, and the Memoir concludes with a statement of the methods which, in the author's opinion, should be adopted in future, for a definitive determination of the solar parallax.

Note on the Durations observed by the French Observers at the Transit of Venus, 1874. By E. J. Stone, Esq.

In the *Comptes Rendus* of the French Academy of Sciences No. 10, March 7, 1881, there is an interesting and valuable paper, by M. V. Puiseux, which contains the results of the contact observations made by the French observers at the Transit of *Venus*, 1874, December 8.

There were eight durations observed by the French observers at five stations: Pekin, Saint Paul, Nagasaki, Saïgon, and Kobé. The observers were Messrs. Fleuriais, Bellanger; Mouchez, Turquet; Janssen, Tisserand; Héraud; and Delacroix. The equations given by M. Puiseux lead at once to the following equations of condition for these eight observed durations:—

$$
\begin{aligned}
1 \quad & x - 2{\cdot}94\,.\,\delta\pi = +0''{\cdot}41, \\
2 \quad & x - 2{\cdot}94\,.\,\delta\pi = +0{\cdot}03, \\
3 \quad & x + 2{\cdot}85\,.\,\delta\pi = +0{\cdot}61, \\
4 \quad & x + 2{\cdot}85\,.\,\delta\pi = +0{\cdot}35, \\
5 \quad & x - 2{\cdot}71\,.\,\delta\pi = -0{\cdot}87, \\
6 \quad & x - 2{\cdot}71\,.\,\delta\pi = -1{\cdot}11, \\
7 \quad & x - 1{\cdot}02\,.\,\delta\pi = -0{\cdot}21, \\
8 \quad & x - 2{\cdot}87\,.\,\delta\pi = +0{\cdot}82.
\end{aligned}
$$

In the formation of these equations the mean equatorial solar parallax has been assumed to be equal to $8''{\cdot}86$, and $\delta\pi$ is the correction which that value requires.

The agreement among these equations is not so close as all astronomers must have wished, and most astronomers may have expected, from the care bestowed upon the instrumental equipment of the different expeditions and the skill of the astronomers employed; but except the equations (5) and (6)—of which more hereafter—it may be seriously doubted whether the discordances are not such as must be accepted in such investigations. The error of each equation is the algebraical sum of the errors made at the two contacts.

If we determine the value of $\delta\pi$ from these equations, as they stand, on the assumptions that all the observations are equally good, and that the results are comparable *inter se*, or free from systematic errors, we shall find for the value of the solar parallax

$$8''{\cdot}96 \pm 0''{\cdot}19\epsilon.$$

Here ϵ is the probable error of the difference between a

distance of the centres of *Venus* and the *Sun*, at contact, and the mean distance between the centres, for all the contacts which are combined in the discussion.

The number of equations combined together is not sufficient for any great reliance to be placed upon the resulting probable error. But with a value of ϵ as large as $0''.5$, or with an assumed probable error of fifteen seconds of time in the observation of the contacts, the theoretical probable error of this result would not exceed $0''.10$; and, unless some of the contacts have been different in kind, such probable errors would be impossible with the optical means at the disposal of the French observers.

The value $8''.96$ is not much larger than that which was obtained from a comparison between the meridian observations of *Mars* made, in 1862, at the Northern Observatories and the Southern Observatories of the Cape and Melbourne; nor is it much larger than the value which will result from a discussion of similar observations made at the same Southern Observatories in 1876. The observations made in 1876 are now under discussion by the authorities at the Washington Observatory, and I had hoped that, long before this time, the result might have been in the hands of astronomers. I have, however, discussed the observations sufficiently to express an opinion that the observations require a value of the solar parallax of about $8''.95$ to satisfy the equations of condition.

I should be prepared to attach very great weight to the value $8''.96$ thus deduced from a discussion of these French durations, were it not clear to me that the equation (5) cannot legitimately be combined in one discussion with the equations (1), (2), (3), (4), (7), and (8), on account of the character of the contact observed at Ingress by M. Janssen.

The following is M. Janssen's account of the contact observed. (*Comptes Rendus de l'Académie des Sciences*, tome lxxx. [1875], p. 344):—

"Dans l'équatorial de huit pouces, dont la lunette est très-bonne, l'image de *Vénus* se montra très-ronde, bien terminée, et la marche relative du disque de la planète, par rapport au disque solaire, s'exécuta géométriquement sans aucune apparence de ligament ni de goutte. Mais il s'écoula un temps assez long entre le moment où le disque de *Vénus* paraissait tangent intérieurement au disque du Soleil, et celui de l'apparition du filet lumineux. Il y a là une anomalie apparente qui, pour moi, tient à la présence de l'atmosphère de la planète. J'ai fait prendre une photographie au moment où le contact paraissait géométrique, et sur cette épreuve le contact n'a pas encore lieu. M. d'Almeida a obtenu une plaque de quarante-sept photographies du bord solaire, qui conduit aux mêmes conclusions. . . .

"Enfin, vers l'instant du second contact intérieur, une éclaircie presque providentielle se produisit sur le Soleil, et nous pûmes, M. Tisserand et moi, prendre l'instant de ce contact, qui fut obtenu avec précision."

This description of the contact observed by M. Janssen at Ingress should be compared with that given by Admiral Mouchez, *Comptes Rendus*, tome lxxx. (1875), p. 616:—

"Mais, à mesure qu'approchait le deuxième contact, les deux parties extrêmes, plus visibles de l'auréole avoisinant le Soleil, tendaient à se réunir en enveloppant d'une plus vive lumière le segment encore extérieur de la planète, et cette réunion *anticipée* des cornes par un arc de cercle lumineux était rendue plus complète encore par un petit rebord très-brillant de lumière terminant l'auréole sur le disque de *Vénus*. Prévoyant dès lors qu'il y aurait une très-grande difficulté, sinon une impossibilité absolue, d'observer le contact géométrique, je changeai vivement le verre de couleur bleu pâle pour en prendre un plus foncé, à l'aide duquel j'espérais éteindre cette auréole et ces lueurs accidentelles, mais ce fut inutilement: l'auréole restant toujours visible, je fus obligé de reprendre le verre primitif.

"Dans de semblables conditions, *je dus prendre comme heure du contact, non pas la réunion des deux cornes ou contact géométrique, mais bien le moment où le disque du Soleil ne me parut plus déformé par la lumière brillante qui enveloppait la planète au point de contact.*

"J'ai noté une différence de temps très-sensible entre l'instant où j'ai cru que ce contact pouvait avoir lieu et celui où j'ai acquis la certitude qu'il avait eu lieu.

"Cette observation me paraît donc comporter beaucoup moins d'exactitude que celle des contacts intérieurs du Soleil et de la Lune dans les éclipses totales ou annulaires que j'ai eu l'occasion d'observer. Dans ces éclipses, en effet, il m'a toujours semblé possible de déterminer, avec la précision d'une fraction de seconde, le moment de la rupture ou de la formation de l'anneau. . . .

"Je dois m'empresser d'ajouter que mon collaborateur M. Turquet, avec un excellent équatorial de six pouces, n'a pas vu l'auréole, et qu'il croit avoir obtenu des contacts d'une grande précision."

At the Egress I saw this "aureole" very distinctly, but when attention was concentrated on the contact I lost sight of it, and it did not interfere with my observation of the contact.

It is clear that the time recorded by Janssen for the contact would have been later by "un temps assez long," had he waited until the disturbance of the illumination of the Sun's apparent limb near the point of contact had ceased, and sunlight had reappeared near the point of contact.

And his Ingress-contact cannot, therefore, be combined with such observations of contact as those made by Mouchez, without a correction for the difference of phase observed. The necessity for such a correction of Janssen's time of contact at Ingress was pointed out by me in the *Notices* for 1876, December, before the accurate times of contact had been published, and when, therefore, it was impossible for me to be influenced by any knowledge

of the value of the solar parallax which would result from a discussion of these observations. The correction clearly required by Janssen's Ingress-contact will make the equation (5) agree more closely with all the other equations except (6), which contains observations made by Tisserand at the same station.

I am unable to find any details of the nature of the contact observed by M. Tisserand. But the residual error of the equation (6) is rather larger than that of the equation (5). It is clearly necessary to reject or correct this equation (5), and I fear, therefore, that it will be necessary also to reject the equation (6). It will be seen that the mean residual error for the equations (1), (2), and (8), with a factor of parallax $-2\cdot9$ is $+0''\cdot42$, whilst the residual error for the equation (6) with a factor of parallax $-2\cdot7$ is $-1''\cdot11$. The residual error of the equation (6), therefore, differs from the mean error of the equations (1), (2), and (8) by no less a quantity than $1''\cdot5$; and from the close agreement of the factors of parallax it is quite impossible to adjust this difference by any change in the adopted value of the solar parallax. Such a difference as $1''\cdot5$ is equivalent to an error of about 45 seconds of time in the observed contacts. It is about equivalent to the whole angular separation between the two phases of the "first appearance of any distinct disturbance of the illumination of the Sun's limb near the point of contact" and the "apparent contact" in a field of considerable brightness; and it would be more than equivalent to such a separation in a feebly-illuminated field where the irradiation would be smaller, and the "apparent contact" at Egress would be earlier and at Ingress later than in a bright field. Speaking from experience, I feel certain that M. Tisserand can never have attempted to observe contacts of the same character as Mouchez. I hope that an examination of the details of his observations will show that he has not attempted to observe at Ingress the "last appearance of any well-marked disturbance of the illumination of the Sun's limb near the point of contact," or at Egress the "first appearance of any such well-marked disturbance," and that it may appear clearly from *a priori* considerations, that his duration cannot be combined with the observations of durations from "real contacts," without a correction for the difference of phase observed.

Rejecting, therefore, on the grounds indicated, (5) and (6), we find from the remaining equations

$$\pi = 8''\cdot88.$$

This appears to be the smallest value of the solar parallax which can be fairly deduced from these observations of duration.

The residual errors of the equations are

$$+0''\cdot15, \quad -0''\cdot22, \quad +0''\cdot25, \quad -0''\cdot02, \quad -0''\cdot50, \quad +0''\cdot56.$$

If M. Tisserand's contact either at Ingress or Egress should clearly appear to be attempts at "apparent contact," then the rejection of both the equations (5) and (6) is absolutely necessary, and the value 8″·88 would be entitled to very considerable weight. On the other hand, should it appear that M. Tisserand's observations are at Ingress and Egress observations of the "last appearance" and "first appearance" respectively of any sensible disturbance of the illumination of the Sun's limb near the points of contact, then the value of the solar parallax given by these durations will be larger than 8″·88; but the probable error must be considerable.

It appears from M. Tisserand's paper in the *Comptes Rendus*, No. 12, 1881, March 21, that the value of the solar parallax 8″·86 found by Le Verrier from a discussion of the masses of the planets requires, if anything, a slight increase rather than a decrease.

The way in which all the more important investigations on the solar parallax have, when cleared from obvious mistakes, given results which group themselves around some such value as 8″·88 is certainly remarkable. The extreme values appear to be those derived from the meridian observations of *Mars* which give some such value as 8″·95, and the values deduced from the recent determinations of the velocity of light, with Struve's coefficient of aberration, which appear to point to some such value as 8″·80. The difference between these values, although greater than could be wished, is not very large, and the mean agrees very closely with the mean of the values which have been obtained from the Transits of *Venus* and from the planetary and lunar theories.

The Partial Eclipse of the Sun, December 31, 1880.

By J. Rand Capron, Esq.

Upon examining the plates taken on this occasion, and described in the *Monthly Notices*, vol. xli. No. 3, p. 144, I have found three appearances in respect of which the photographic image differs from that seen by the eye, either unaided or with the telescope. As these are probably always, in a greater or less degree, present in photographs of similar objects, it may be useful to describe them.

1. Round the edge of the solar image, and in a stronger degree where it touches the Moon's limb, is the bright line of light seen in most photographs when black is contiguous to white. In portraiture it is well shown, edging a velvet dress, and is then frequently very sharp and definite. This effect—generally termed "photographic irradiation"—has been attributed by Lord Lindsay and Mr. Ranyard to an extension of chemical photographic action on the plate. Others, I believe, have attributed it to a thickening or building up of the chemicals

parallel to the planet's belts. The declination wires were then separated till one of them coincided with the planet's eastern limb and the other with the western. In this way a determination of the equatorial diameter of the planet was obtained uncorrected for defective illumination. The wires being then set to half the measured diameter, the western wire was brought on the corresponding limb of the planet, and the eastern wire assumed to bisect perpendicularly the visible equatorial diameter. The wires being retained in this position, the transits were observed over the latter wire. Immediately on noting the coincidence of each part of the spot with the bisecting wire the position of the wires was changed by bringing the eastern wire on the planet's corresponding limb, and the western wire became thus in its turn the bisecting wire. The observed parts of the spots being again found coincident with the bisecting wire, the observations were regarded as verified. It will be seen that my estimated times of transit are in every instance later than those noted with the aid of the micrometer. The micrometer, which is the work of Messrs. Cooke and Sons, of York, the makers of the telescope itself, appears to be an excellent one as regards the screws, the value of one revolution being 29″·717. Each wire, however, I find from numerous observations to be about 1″·2 in thickness.

I have given the observed transits to the nearest tenth of a minute of local mean time, as it is impossible to observe with anything like precision. The impossibility of doing so is obvious from the recorded results of other observers.

Date of Transit.	Magnifying Power.	Transits, per Micrometer.			Transits, per Estimation.		
		Preceding End.	Middle.	Following End.	Preceding End.	Middle.	Following End.
1880.		h m	h m	h m	h m	h m	h m
Oct. 31	180				8 35·6	8 58·3	9 28·6
Nov. 2	180				10 6·4	10 34·4	11 2·4
7	180				9 19 8	9 41·9	10 12·0
10	187	6 30·4	6 58·6	7 35·8	6 41·6	7 5·6	7 41·3
15	55				6 0·7	6 25·0	6 49·2
17	187	7 21·8	7 48·3	8 14 8	7 30·0	7 56·3	8 25·3
24	187	8 8·6	8 35·7	9 3·4	8 16·9	8 44·4	9 8·7
Dec. 1	187	8 54·3	9 19·7	9 49·3	9 0 0	9 24·8	9 53·3
4	180				6 37·9		7 26·3
10		11 12·6	11 45·1	12 17·1	11 20·9	11 49·5	12 19·6
18	187	7 55·9	8 23·7	8 52·2	8 0·4	8 28·9	8 54·7
30					8 0·2	8 22·5	8 47·7

Observatory, Windsor, N.S. Wales:
1881, *January* 28.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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J. R. HIND, Esq., President, in the Chair.

John Alfred Barringer, Esq., Ackworth School, near Pontefract, Yorkshire;

Thomas Percy Gray, Esq., The Lodge, Clapham Road, Bedford; and

Robert Wigglesworth, Esq., St. Michael's House, Minster Yard, York,

were balloted for and duly elected Fellows of the Society.

On the Inclination of the Zodiacal Light. By T.W. Backhouse, Esq.

I do not know whether the inclination of the Zodiacal Light to the ecliptic has been calculated before, but even if it has, an independent investigation will not be without its value.

With this view, I have examined my observations of the position of the central line or axis of the Zodiacal Light from 1867 to the summer of 1877. There are altogether 418 observations of various points of the central line (counting those most roughly observed as but half an observation): 360½ give northern latitude, average + 2°·32; 33½ give the latitude as 0°; and 24 give southern latitude, average −0°·94. The average of the whole is + 1°·95; and the average deviation from the ecliptic is 2°·06.

I have arranged the observations according to their distance from the Sun, and according to the Sun's longitude. A summary of them is given in the accompanying table, which shows, in degrees, the average latitude of the axis for every 30° of the Sun's longitude, and every 30° of distance following the Sun. The number of observations is given below to the left of each average.

TABLE I.

Longitude of Zodiacal Light following the Sun.

Sun's Long.	330° 345–316	300° 315–286	270° 285–256	240° 255–226	210° 225–196	180° 195–166	150 165–136	120° 135–106	90° 105–76	60 75–46	30° 45–16	Aver. and Total.
146–175			+2·0	−0·0	−1·2	−1·0						
			1	4	4	1						
176–205	+2·2	+1·3	+0·3		+1·8	+0·4	+0·4					
	2	11	3		22	27	10					
206–235	+3·2	+2·7	+1·9		+3·0	+2·0	−0·1	+0·0				
	6	9	4		1	8½	7	1				
236–265	+3·0	+4·0	+3·1	+1·7		+2·5	+1·0	+1·3	+2·8	+2·0		
	1	7	4	4		3	7½	9	8½	1		
266–295		+3·7	+5·5		+1·5		+1·7	+2·2	+1·9	+2·7		
		2	2		1		2	8½	12½	2½		
296–325					+1·3	+1·6	+1·5	+2·8	+2·2	+1·9	+2·4	
					3	5	1	5	23½	19½	3½	
326–355				+3·0	+2·9	+1·9	+2·1	+1·7	+2·6	+2·4	+2·0	
				1	5½	20½	11½	3	13½	35½	7½	
356–25					+3·0	+2·2	+2·6	+1·4	+1·5	+2·5	+2·6	
					1	11½	10½	7	1	10½	5½	
26–55						+3·0	+3·6	+2·8	+2·4			
						2	5	4	4½			
Average	+2·9	+2·5	+2·4	+1·1	+1·7	+1·4	+1·6	+1·9	+2·3	+2·3	+2·3	+1·59
Total	9	29	14	9	37½	78½	54½	37½	63½	69	16½	418

On the theory that the Zodiacal Light is a solar appendage, of lenticular form, decreasing in brightness from the Sun; that its central plane maintains a constant position; and that the counter-glow is not a physical feature, but an apparent phenomenon, causing an increase of brightness at the point opposite the Sun—this table should present greater regularity than it does: only that, as the Zodiacal Light is so very indefinite, and observations of its central line therefore so inexact, we should expect the errors of observation to be proportionately very great. On this account, it is necessarily difficult to arrive at a correct idea of the inclination. First, I endeavoured to find the longitude of the ascending node, in the following different ways:—

A general examination of the table here given, ignoring those cases where the number of observations is small, seems to indicate that its most probable position is at long. 30°.

If the assumptions given above are correct, twice in the year the Earth must be in the axis of the Zodiacal Light; and at these times the latitude will be 0° at and opposite the Sun, and will reach its maximum, *plus* and *minus*, 90° off it. Careful examination of several tables shows that this is the case at no time of the year; the average position of the Sun agreeing best with this is long. 232°, giving the ascending node at long. 52°.

Using only those few observations that seemed likely to be the best on account of their great altitude, and confining my attention to those few of these that were about 90° off the Sun, on account of their possessing a fair brightness, the most probable longitude of the ascending node comes out at 55°.

On the hypothesis of the counter-glow with which I began, the central line there will be nearer the ecliptic than the true position of greatest density of the Zodiacal Light; how much nearer, will depend on the relative rates of concentration of the Zodiacal Light itself, and of the law determining the counter-glow. From the best observations of the counter-glow, the ascending node is at 37°.

The average of all these results is 43°.

But probably the best mode of examining the observations is to see whether the latitude of each point in the central line rises and falls among the stars in the course of the year, as it should do if the theories with which I started are correct. By this means any false results which might arise from constant errors, such as might be caused by the brightness of neighbouring stars or the presence of unsuspected branches of the Milky Way, will be got rid of. For this purpose, a table has been prepared, showing the way in which the central line should rise and fall according to theory; and I have compared the observed latitude of each point of the central line with this theoretical table, to see with what longitude, with reference to the ascending node, it would agree best. The results are shown in Table II., the first column of which gives the average longitude of the portion of the Zodiacal Light examined, and the second column the longitude, with refer-

ence to the ascending node, with which its variations agree best. The third column gives the longitude of the ascending node resulting from this.

TABLE II.

Long. of Zodiacal Light: Average Position Observed.	Agrees best with Ω +	Ascending Node.
°	°	°
340	312	28
350	About 35	About −45
355	−15	10
11	−14	25
16	About 0	About 16
25	345	40
35	25 to 75	10 to −40
44	22, rather badly.	22
50	70, variations slight.	−20
62	30	32
111	67	44
126	About 100, variations slight.	About 26
147	100	47
164	90, very badly.	74
178	135, rather badly.	43
188	145	43
200	150, rather badly.	50
215	About 180, badly.	About 35
		Average 27

The best average of this table is 27°, which, combined with the average of the previous modes of examination, gives

$$\text{Long. ascending node} = 35^\circ.$$

These results seem sufficiently consistent to give considerable probability, though I think not amounting to proof, that the hypotheses on which we started are in the main correct, and that the ascending node of the Zodiacal Light is somewhere about long. 35°.

The inclination is more difficult to determine. Theoretically, it would be very simply determined from the observations of the Zodiacal Light at the time when the Sun's longitude is 35° or 215°; but these are so irregular that it is impossible to ascertain it exactly. These observations are best satisfied by

$$\text{Inclination} = 1^\circ{\cdot}7;$$

but this requires the hypothesis of a constant error in all the observations, amounting to + 1°·3. Without any such hypothesis, the inclination comes out 2°·9, but in this case the probable error of the observations is much larger. It is obvious that either result is entitled to very little weight, the probable error in any case being very large.

If the Zodiacal Light is a solar appendage, we should not be surprised at finding its axis in the plane of the solar equator; but it is quite evident that such is not the case. We should have no reason for expecting it to be in the plane of the ecliptic rather than in that of any other planet's orbit; it would be more likely to be in the plane of the average position of the orbits of all the planets. But I have not calculated what this plane is, so as to avoid every bias in my investigations as far as possible.

Except on two nights, all the observations were made at Sunderland, lat. 55° N.

I have been aided in the calculations by J. Taylor, E. Dodshon, A. Gröne, and A. J. Edmunds.

Sunderland:
1881, *April* 27.

Note on the Flexure of the Greenwich Transit Circle.

By W. H. M. Christie, Esq.

Prof. Newcomb has recently called my attention to the practical difficulties attending the determination of the astronomical flexure by means of opposing collimators, and to the effect which the increase in the temperature of the air from the bottom to the top of the observing room might have on this observation. And he points out that if no change had been made in the adopted flexure at the time of the piercing of the cube of the Transit Circle, there would have been no striking change in the R−D correction. In his work on the "Declinations of Fixed Stars," pp. 35 and 36, Mr. Lewis Boss has examined the flexure and R−D corrections 1851–1872 with reference to Prof. Newcomb's suggestion, and has found that if the uniform flexure +0″·21 had been employed throughout the series, we should have had sufficiently good agreement between direct and reflection observations. This view entirely agrees with the conclusion at which I arrived in my paper on the Systematic Errors of the Greenwich N.P.D.'s from the discussion of the observations of circumpolar stars and of the Sun, before I was aware of Prof. Newcomb's suggestion or of Mr. Boss's work.

As further data are now available, I have thought it might be not without interest to compare the flexure and R−D corrections for the period 1851 to 1880. The following table is derived from that which I gave in the Addendum to my paper

(*Memoirs*, vol. xlv. p. 182) with the addition of the R—D correction for 1880—viz.

$$+0''.07 + 0''.54 \sin z \cos^2 z,$$

which has been recently computed.* I have converted the corrections from the law $b' \sin z \cos^2 z$ to $b \sin z$ by multiplying b' by ·843. The R—D correction is thus the correction apparently required to the Adopted Flexure.

	R–D $b \sin z$.	Adopted Flexure.	Apparent Flexure.	Weight.
1865	−0″.65	+0″.76	+0″.11	1
1861–64	−0·45	+0·56	+0·11	4
1857–60	−0·39	+0·56	+0·17	4
1851–56	−0·29	+0·58	+0·29	6
1879–80	+0·39	0·00	+0·39	2
1871–78	+0·43	−0·12	+0·31	8
1866–70	+0·65	−0·37	+0·28	5

Under the head "Apparent Flexure" is given the flexure correction indicated by the reflection observations of stars, found by adding the coefficient of the R—D correction to the Adopted Flexure.

Taking the means of the Apparent Flexure for the whole period and for the periods before and after the piercing of the cube, we have—

Mean Flexure	1851–1880	...	...	+0″.25
„	1851–1865	...	...	+0·20
„	1866–1880	...	...	+0·31

From this it would seem that if any change of flexure occurred on the piercing of the central cube of the Transit Circle, it was in the opposite direction to that indicated by the Collimator determinations, and was simply a small increase in the coefficient. With regard to the apparent change of sign indicated by the collimators, it is to be remembered that before the piercing of the cube the observations were made with collimators of four inches' aperture and with the instrument raised, and that after the piercing they were made with collimators of seven inches' aperture and through the openings in the cube. Now it is found that in the determination of collimation-error there is a discordance between the readings taken with the instrument raised and through the cube respectively, and this difference affects the result for collimation-error in the mean by 0″·30. If abnormal refraction resulting from inequalities of temperature has anything

* The Adopted Flexure for 1851 and 1852 was really 0″·73, instead of 0″·50. I have here corrected for this error.

to do with this discordance, it seems not unnatural to suppose that it might produce even a larger effect in zenith distance. No such discordance, however, has been detected, though some observations with the instrument raised, and through the cube, have been made for this purpose.

The recent determinations of flexure by the collimators do not, however, differ much from that indicated by the R—D correction. The following are the results since the beginning of 1879:—

1879, April 22	+0″·26	1880, July 22	+0″·04
23	+0·07	Dec. 6	+0·20
1880, April 6	+0·19	1881, May 2	+0·13
May 6	+0·04	May 10	+0·18

The mean of these is +0″·14, whilst the mean from the R—D correction is +0″·25.

With reference to the question of the distribution of temperature in the room, the following readings of thermometers mounted respectively near the north and south collimators and the central cube, as well as of the exterior thermometer, were taken on 1881 May 4 and on May 10 at the beginning and end of the observations:—

	N. Coll.	Cube.	S. Coll.	Exterior.
h	°	°	°	°
May 3 22	49·6	49·8	49·8	50·8
9 22	47·8	48·3	47·5	47·9
9 23	48·6	48·9	48·6	49·1

The readings of the thermometers were inadvertently omitted on May 2; those on May 4 are given as illustrating the distribution of temperature under nearly similar conditions. Considering the difficulty of determining the true temperature of the air in the neighbourhood of large masses of iron and stone, the readings of the interior thermometers seem to correspond as closely as can fairly be expected.

In conclusion, I must point out that although the R—D discordance may reasonably be explained as the effect of error in the flexure determination, there yet remains to be accounted for the error of nearly 1″ at Z.D. 60° North, which is indicated by the circumpolar observations 1868–1876, and this is to my mind a much more serious matter.

Blackheath:
1881, *May.*

Further Remarks on Mr. Stone's Alterations of Bessel's Refractions.

By W. H. M. Christie, Esq.

Mr. Stone, in his Reply to my paper "On Mr. Stone's Alterations of Bessel's Refractions," published in the March No. of the *Monthly Notices*, vol. xli. pp. 272–281, has tacitly conceded so many of the points which I have urged that I should hardly have considered it necessary to say anything further on the subject if it had not been that he asks for explanations of some of my statements.

With reference to paragraphs 1 and 2 in the remarks on p. 95 of my paper, I can only express my regret that they have been misunderstood. They were not intended to cast any reflection on Mr. Stone, but to call pointed attention to the omission of a number of circumpolar observations from the collected results in the Latitude Investigation given in the Greenwich "Introduction." With the exception of the years 1857, 1858, and 1859, the invariable practice has been, I believe, to collect all the results of circumpolar observations in the Latitude Investigation. As I am not the only person who has felt a difficulty from the absence of all explanation in Mr. Stone's original paper on Bessel's Mean Refractions of the source from which he derived his results, and as others might be misled by the omission of the observations from the Greenwich "Introductions," I left the statement of my difficulties as it originally occurred to me, and gave the explanation afterwards. I should be sorry to think that my statement would bear such a construction as Mr. Stone seems to put on it. It was meant to be explanatory, not critical, for it is obvious that serious criticism would be wasted when Mr. Stone himself is reduced to saying "I have felt throughout that I must have applied all the corrections which could be applied with certainty to the results." Even now Mr. Stone does not say whether he used the old law for R—D, or the new, or a mixture of the two. Mr. Stone had shown to his own satisfaction that his results were right and mine were wrong, and under these circumstances I desired to explain that there was nothing incompatible in two sets of results obtained with different data and different corrections. I do not attach the same importance to these particular observations that Mr. Stone does, and I regard them as simply a small portion of the chain of evidence on the R—D correction and Bessel's Refractions.

The next point that calls for explanation has reference to Mr. Stone's remarks on pp. 275, 276, in which, after imputing to me a statement which I have not made, and which, in fact, conveys a totally different meaning from my actual words, he proceeds to call attention to a serious fallacy which invalidates my work upon these questions of refractions. The fallacy in question is to be found in the words which Mr. Stone puts into my mouth, but not in what I have actually said. Mr. Stone

argues that *Bessel's Refractions unaltered* do not represent the Greenwich observations, because, as Mr. Main has shown, alterations are required beyond Z.D. 82°. This is perfectly true, and, as Mr. Stone well knows, I have over and over again stated that I adopt Mr. Main's corrections at very low altitudes, considering that they show Bessel's *law* of refraction to be erroneous. This, however, does not in the least affect the question of the accuracy of Bessel's *constant* of refraction, or, as it is sometimes expressed, Bessel's *mean* refractions. Mr. Stone's argument, therefore, falls to the ground unless he can show that I have maintained that *Bessel's Refractions unaltered* represent the Greenwich observations. He quotes me thus, p. 275 (the italics are mine):—

"Mr. Christie considers my statement 'that the accumulated evidence of the Greenwich circumpolar observations is against the accuracy of *Bessel's Refractions unaltered*,' as 'really quite misleading.'"

What I really said was this, under the heading "First, as regards the value of the *constant* of refraction," on p. 97:—"In the first place, he [Mr. Stone] brings forward his discussion of the observations 1857–1865, and appeals to his residuals and to the accumulated evidence of the Greenwich circumpolar observations in favour of his diminution of the *mean refractions*;" and further on in the same paragraph I add:—"So that it is really quite misleading to speak of the accumulated evidence of the Greenwich observations." The reference here is to Mr. Stone's statement in his paper "On *Mean* Refractions," April 1880, *Monthly Notices*, vol. xl. p. 336:—"The recent Cape Observations do appear to offer grounds for serious doubts about the necessity of decreasing Bessel's Refractions, *for small zenith distances*, by 0·0053; but the evidence thus collected is not certainly such as should outweigh the accumulated evidence of the Greenwich circumpolar observations."

There is another statement made by Mr. Stone which does not seem to be characterised by the accuracy which is desirable in such matters. On p. 276 he makes this statement: "With respect to the periods 1851–1861, 1862–1865, Mr. Christie has unfortunately grouped together observations reduced with Bessel's Refractions and observations reduced with refractions obtained by throwing away from Bessel's Refractions quantities varying from 0″·9 to over 2″." This statement is incorrect as regards the facts, and is calculated to convey the erroneous impression that I had not applied corrections to reduce the observations to the same set of refractions throughout. In the first place, the actual corrections range from 0″·9 to 1″·12, *not* "from 0″·9 to over 2″," and the appropriate correction has been applied to each individual result, so that there is no error from this cause; and, in the second place, the observations 1862–1865 have *all* been reduced with the same refractions—viz. Bessel's with Main's corrections below Z.D. 82°. It is curious that Mr. Stone should himself have done the very thing for which he reproaches

me, for he has grouped together observations above 82° Z.D. and observations below 82° Z.D. in his last group N.P.D. 43° to 46° 29′, and, what is a more serious oversight, he has grouped together observations reduced with two different laws for R−D discordance, without, as far as I can make out, applying any correction for the difference, which amounts to 0″·36. With reference to my selection of the groups of years 1851–1861 and 1862–1865, I may explain that one of the principal objects of my paper on the Systematic Errors of the Greenwich N.P.D.'s was to examine the validity of the R−D correction, and that the law was changed at the beginning of 1862. I did not propose to myself any discussion of refractions at low altitudes, and the examination of the results which Mr. Stone obtained in 1867 from the observations 1857–1865 was quite an incidental matter. Had I intended to discuss the refractions below 80° Z.D., I should have adopted Mr. Main's exhaustive treatment rather than the method followed by Mr. Stone, and I should have included in the discussion the Greenwich circumpolar observations below Z.D. 85°, of which in the period 1857–1865, there were 33 above the Pole and 21 below. It would be interesting to know why Mr. Stone has omitted these observations, which have an important bearing on the special question which he discusses.

But though I do not profess to discuss the refractions at very large Z.D., for which the data accumulated since Mr. Main's Memoir seem hardly sufficient, I must take exception to Mr. Stone's assumption that observations extending to Z.D. 78½° are not sufficient for discriminating between Mr. Stone's *constant* of refraction and Bessel's. As a matter of fact, Mr. Stone's alteration would affect the residual at Z.D. 78½° by 1″·0. But my reason for not discussing the observations below Z.D. 78½° in the period 1840–1847 (I have gone as far as Z.D. 84° in the periods 1851–1861 and 1862–1865) is that I did not see that I could add anything to Mr. Main's exhaustive discussion of all the observations below Z.D. 70° in the period 1836–1854. As Mr. Stone studiously ignores my reference to this important investigation as supplementing my own work, I will here give the residuals obtained by grouping Mr. Main's results for individual stars, in connection with my residuals for the periods 1840–1847 and 1851–1861:

N.P.D. above Pole − N.P.D. below Pole.

With Bessel's Refractions and Main's Corrections below Z.D. 82°

Mean N.P.D.	2°	7½°	12°	21°	28°	31°	33°	37°	41°	44½°
α	+″·15	+″·05	−″·07	−″·18	−″·07	+″·02	+″·37	+″·20	″	″
β							+·13 ±·13	+·22 ±·13	−·22 ±·16	−·09 ±·16
γ	−·10	+·02	+·04	−·27		+·15		+·04	+·02	−·02

The residuals α and γ are for the periods 1840–1847 and

1851–1861; the residuals β are formed from Mr. Main's results, *Memoirs R.A.S.* vol. xxvi., Table viii. pp. 122–23, giving weights by the formula $W=\frac{1}{2\epsilon^2+\frac{e^2}{n}}$, where e is the probable casual error of an observation S.P., and ϵ (which is taken $=0''.22$) is the probable error of the result above pole and the probable systematic error of an observation S.P. The probable error of the residual inferred from the weight of the group is appended with the sign $\pm$. Each of the groups here treated corresponds to two of Mr. Main's groups in his Table x. (cont.), p. 129.

I submit that these residuals afford stronger evidence in favour of Bessel's constant of refraction than those from the observations 1857–1865 do in favour of Mr. Stone's diminished constant. And I cannot help thinking that he should have weighed the evidence of these 19 years, from 1836–1854, a little more carefully before he proposed a change which has turned out so unfortunately. It is to be noted, too, that his residuals B are seriously affected by the assumed law for the R – D correction, which I have given reasons for considering to be erroneous.

As regards his residuals C, 1868–1876, I must ask why he omits to give the weights of the several groups. I should imagine that no practical astronomer would accept Mr. Stone's residuals without inquiring what are the weights of the respective groups, what corrections have been applied, and what residuals would be given by an alternative hypothesis. The importance of this last test I have illustrated by reference to Mr. Stone's discussion of the Melbourne circumpolar observations* as well as of those now in question. In both cases I have shown that Bessel's Refractions with Main's corrections below Z.D.82° give smaller residuals.

There is only one point which calls for further remark, and that is with reference to the adopted co-latitude, for the years 1868–1876. Here Mr. Stone has apparently misapprehended my remarks and has so paraphrased my words as to convey a very different meaning. What I said was, not that the apparent change in the co-latitude "is due *solely* to a better distribution of the weights," but that it "is simply due to a change in the system of weights *and* to the increased number of observations of wide circumpolars," and I add that it began "in 1872, when special attention was given to the observation of *wide* circumpolars." No doubt it seems very specious to use the co-latitude given by the observations, but if this effective co-latitude does not agree with the real co-latitude of the Greenwich Observatory, it seems to me to show that there is a systematic error in the observations, which we ought rather to try to detect than to

* There is a small printer's omission in my paper, *Monthly Notices*, vol. xli. p. 98, line 31: "Each has a probable error corresponding to weight,' should read, "Each has a probable error corresponding to weight 1, the figure 1 having dropped out in printing.

mask by the method which Mr. Stone adopts. And I would point out that Mr. Stone does not use what he terms the effective co-latitude—*i.e.* the difference between the circle-readings for the zenith point and the polar point—for that can only be given by observations of stars close to the polar point and not by stars 20° from this point. As I have pointed out, the close circumpolar stars show no change in the co-latitude, and thus there is no change in the difference of circle-readings for the zenith point and the polar point of the circle. But there is a change of nearly 1″ (as the curve given in my paper "On the Systematic Errors of the Greenwich N.P.D.'s" shows) in the difference of circle-readings for the zenith point and a point 22° below the pole at Z.D. 68° north. And this has nothing to do with the adopted zenith points, since the same change is shown in the distance between the polar point and the point 22° below the pole. Mr. Stone's argument that if the method of correcting the nadir point was perfectly satisfactory, then the co-latitudes determined from the observations should be perfectly satisfactory, amounts to this, that if one thing is right other things cannot be wrong. I am glad, however, to find that Mr. Stone does not now dispute the validity of the method of correcting the nadir-point observation.

I now come to the statement which I gave respecting the errors of the micrometer screws, and which Mr. Stone thinks is not made with the precision which is desirable.

The statement in question—"In 1868, at Mr. Stone's suggestion I believe, the practice was commenced of taking the nadir point at 0^r of the microscope-micrometers"—means that the following notice was put up in the Transit Circle room: "The Nadir Point observation is to be taken at 179° 40′ + 0^r." As Mr. Stone is, I believe, responsible for this notice, no doubt he can give its precise meaning. In my paper "On the Effect of Wear on the Micrometer Screws of the Greenwich Transit Circle" (*Monthly Notices*, vol. xxxvii. p. 20) it was sufficient for me to show that there was a preponderance of a large number of observations between 0^r and 1^r of Microscope A as it read in 1875, or between $0^r{\cdot}2$ and $1^r{\cdot}2$ of Microscope A as it read during a considerable part of 1868. An examination of the curves I have given will show that when the corrections indicated in the text (p. 20) are applied to the 1868 curve (viz. −0″·4 approximately at 0^r, and +0″·2 at 5^r, with intermediate corrections between these two points) the 1875 curve begins to rise above the 1868 curve (indicating increased wear of the screws) at about $0^r{\cdot}4$ of Microscope A as it read in 1868, and does not meet this curve again till about $1^r{\cdot}2$. It must be understood, however, that no great precision is to be expected in such a case, on account of accidental errors in the curve, and I have therefore contented myself with specifying the part between 0^r and 1^r as indicating extra wear since 1868. I have nowhere said anything about readings of Microscope A less than $0^r{\cdot}2$, and therefore Mr.

Stone's interesting collection of such readings does not in the least concern me. Let us now take the nadir observations 1868, Feb. 3, to 1869, Dec. 31, with reference to my actual statement, and not with reference to Mr. Stone's interpretation of it. In the period cited there were 559 determinations in all: 543 of these were made at readings (of Microscope A) $1^{r}\cdot2$ and under, 520 at readings $1^{r}\cdot0$ and under, 488 at readings $0^{r}\cdot9$ and under, 448 at readings $0^{r}\cdot8$ and under, 334 at readings $0^{r}\cdot6$ and under, and 283 at readings $0^{r}\cdot4$ and under. I think it is evident from this that there was a large preponderance of observations in the part of the screws I have specified, more especially since the nadir-point observation was usually taken twice a day in the latter part of the period 1868–1875. It seems a pity that Mr. Stone did not make himself better acquainted with the facts before making the confident assertion that "whatever may have been the state of the screws near $0^{r}\cdot0$ in 1876, it certainly was not caused by the 4,000 observations made near $0^{r}\cdot0$ at my suggestion."

With regard to the practice introduced in 1868 of taking the nadir point at 0^{r}, Mr. Stone states that it was done to avoid very large corrections for run. I had heard this reason given, but I should have been loath to attribute it to Mr. Stone, for it implies the serious fallacy that the real reference point for runs is 0^{r} instead of being about $2^{r}\cdot5$, the middle of the range over which the observations are distributed. It is perfectly true that for mere convenience of computation the correction for runs is reckoned from 0^{r} so as to avoid negative signs, but the real correction for runs, as affecting any particular observation, is the difference between the correction reckoned from 0^{r} and the average correction for observations of stars. I had stated this in the very passage which Mr. Stone quotes, and had given it as the reason for my suggesting that the nadir observation should be taken about $2^{r}\cdot5$, but as Mr. Stone takes no notice of it, I am obliged again to call attention to the point.

After this statement of Mr. Stone's I am not so surprised as I should otherwise have been at the difficulty he experiences in understanding how the corrections for runs enter into the corrections for errors of micrometer screws, though I thought I had sufficiently explained the matter in my paper of 1876. Mr. Stone is quite right in supposing that the runs had not been corrected for errors of screws before their application, but he is quite wrong in supposing that there was any oversight in the matter, and by his implying that the runs ought to have been so corrected for errors of screws he seems to me to show that he does not appreciate the nature of the problem.

The problem really was this: To determine the corrections applicable to the Greenwich N.P.D.'s on account of errors of the micrometer screws, these N.P.D.'s being affected by error of the screws at the point at which the observation was made, and by the error in the runs resulting from error of the screws at 0^{r} and

4ʳ·9. For this purpose simultaneous observations were made with the ordinary micrometers, and with the four supplementary micrometers, the latter being presumably free from error. The observations with the ordinary micrometers were reduced with the runs derived from these worn screws as in the ordinary course of work, and the observations with the supplementary micrometers were reduced with the runs from their own screws. By the comparison of the two sets the curve was laid down, "the ordinates of which give the corrections to the circle-reading corresponding to the revolutions and tenths of Microscope A" (*Monthly Notices*, vol. xxxvii. p. 19).

There is no oversight in this. The method was adopted advisedly with the object of determining, not the actual errors of the micrometer screws, but the corrections to the circle-readings on account of these errors. The determination of the actual errors has no practical importance, and could not have been accomplished without special apparatus and a serious interruption to the regular observations. And even if the actual errors had been determined it would have been necessary to correct the runs as well as the micrometer-readings for every observation, and these two corrections would together be equal to the single correction actually applied.

There may, however, be a small effect from error in the runs, if the runs be taken at different parts of the screws. I have expressly referred to this in my paper of 1876 as probably explaining a small difference between the errors at 0ʳ and 5ʳ in 1868 and 1875 respectively. Since the correction for runs is adopted from a considerable number of determinations extending usually over a fortnight, it may safely be assumed that the adopted run will apply sensibly to the same part of the screws.

As regards the other remarks which Mr. Stone makes on the alterations of Bessel's Refractions, I do not find that he has brought forward any new points which call for further notice after the somewhat lengthy discussions of Bessel's constant and law of refraction contained in my two previous papers on the subject.

Blackheath:
1881, *May.*

On the Supposed Difference in the Refractions North and South of the Zenith of Melbourne. By A. M. W. Downing, Esq.

In the *Monthly Notices* for December 1867 there is a paper by Mr. Stone "On Bessel's Mean Refractions," containing (amongst other things) a discussion of the North Polar Distances of stars observed at Melbourne during the years 1863, 1864, and 1865. As the result of this discussion it appeared that from the North Polar Distances of stars observed above and below the pole, the refractions of the *Tabulæ Regiomontanæ* should be diminished by 0·00372 × Refraction; whilst from the comparison

of Greenwich and Melbourne results it appeared that, to the north of the zenith the tabular refractions should be diminished by 0·00914 × Refraction, to satisfy the Melbourne observations. The correction to the adopted co-latitude, corresponding to the above diminution of the tabular refractions derived from the observations of circumpolar stars, was +0″·149. These results were actually used in the reduction of the observations of stars whose places are given in the First Melbourne General Catalogue for the epoch 1870, it being assumed that the mean refractions were really different north and south of the zenith. A discussion of the same Melbourne observations by Prof. Gyldén (*V. J. S.* Bd. iv. Heft 2) was considered to be confirmatory of Mr. Stone's results, and also showed that* "a similar discrepancy was apparent in the Cape of Good Hope and the St. Helena observations, thus indicating the cause to be a general one." With regard to the Cape of Good Hope observations, it must be remarked that Prof. Gyldén's investigations only refer to the observations made with the old Mural Circle, and that Mr. Stone has shown (*Monthly Notices*, Dec. 1879) that, as far as the observations made under his direction are concerned, there is no sensible difference between the mean refractions north and south of the zenith, the observations on both sides being very fairly represented by Bessel's Refractions unaltered. And I have no doubt that if a first-class Transit Circle were erected on the site of Johnson's Observatory at St. Helena, we should have very similar results.

My attention was drawn to the large correction which has been applied to the refractions of the *Tabulæ* in the formation of the N.P.D.'s of the Melbourne Catalogue for stars north of the zenith by finding, from a discussion of the star places of the Catalogue (*Monthly Notices*, Jan. 1879), that the N.P.D.'s of stars passing the meridian near the north horizon appeared to be systematically too large, and therefore that the adopted refractions were too small. The same thing appears from a comparison of the Melbourne and Washington observations made by Mr. Lewis Boss ("Declinations of Fixed Stars," p. 67), from which he finds that the refractions of the *Tabulæ* must be diminished by 0·00733 × Refraction to satisfy the Melbourne observations. (With regard to the assumption of a difference in the refractions north and south of the zenith, Mr. Boss aptly remarks that, "though possible on *a priori* grounds, this hypothesis is open to serious objections, when we consider the difficulty of determining the correction to the adopted refractions independently at the latitude of Melbourne.") There cannot be much doubt, therefore, but that the correction applied to the stars on the north side, derived from the observations made 1863–1865, is too large to satisfy the observations made during the whole period 1863–1870, the results of which are embodied in the Melbourne Catalogue. Under these circumstances it becomes a matter of interest and importance to investigate the subject in as complete a manner as

* Introduction, *Melbourne Cat.* p. iv.

the available materials render possible; and I have therefore not hesitated to avail myself of the opportunity for entering on the discussion presented by the publication of another volume of Melbourne Astronomical Results.

This volume (the fifth of the series) contains the results of observations made during the years 1871–1875, both inclusive. The instrument and methods of observing remain substantially unaltered, and therefore need no detailed description here. It may, however, be remarked that special attention has been paid to the observation of wide circumpolars, and of stars passing the meridian near the north horizon, for the purpose of determining the refraction both to the north and to the south of the zenith. There is unfortunately no provision for the observations of stars by reflexion. This is a most serious omission, and must greatly diminish our confidence in the Melbourne results. Reflexion observations are most essential, both for the accurate determination of zenith points (in combination with the nadir observation), and also for detection of systematic errors in the circle-readings, whether these arise from uncorrected flexure or other causes. I hope that before long we shall have reflexion observations made regularly and uninterruptedly at Melbourne.

With regard to the horizontal flexure, it is stated in the Introduction to this volume that its amount was determined, by the method of opposing collimators, once a week from the beginning of 1871 to the end of 1874, and once a month during 1875; a mean value was used for the interval between the observations, as in the case of the runs. It is unfortunate that no table of these observations, or of the results derived from them, is given, so that we have no information as to the stability or otherwise of this important correction. Those who are concerned with the printing of astronomical observations would do well to remember that it is very desirable that all matters relating to the adjustment or stability of instruments should be given in considerable detail.

I now proceed to the discussion of the observations of circumpolar stars contained in this volume. The places of the stars as found from each year's observations have been taken from the Annual Catalogues, and reduced to a common epoch 1873·0, the results for upper and lower transit being of course kept separate; mean places have then been computed for each star for 1873·0, and the difference N.P.D. at upper transit *minus* N.P.D. at lower transit found. The stars have next been arranged in order of N.P.D., and divided into groups, each group embracing about 5° of N.P.D. The results of each group have been combined, so that corresponding to a definite N.P.D. we have a definite discordance, and for the purposes of the present investigation it is assumed that these discordances arise from error in the adopted co-latitude, and error in the adopted refraction. The co-latitude adopted in this volume is that originally found for the Melbourne Observatory—viz. 52° 10′ 6″·6—and the refractions used are those of Bessel's *Tabulæ Regiomontanæ* unaltered. The weights

assigned to the places of individual stars are found from the expression for the probable error of a Melbourne result given by Prof. Gyldén in the paper referred to above (*V. J. S.* Bd. iv. 102), viz. :—

$$\epsilon^2 = (0\cdot26)^2 + \frac{1}{n}\left\{0\cdot263 + 0\cdot0467\left(\frac{\text{refr.}}{100}\right)^2\right\},$$

where n is the number of observations on which the result depends. The weight of a discordance N.P.D. upper *minus* N.P.D. lower for a single star is

$$w = \frac{1}{\epsilon_U^2 + \epsilon_L^2},$$

and the weight to be assigned to a group

$$W = \Sigma w.$$

The following table gives the discordances and the corresponding N.P.D.'s found in the manner just described; and I have added for comparison the results obtained by applying the corrections found by MM. Stone and Gyldén from their discussion of the observations 1863–1865.

Mr. Stone's corrections are given in a tabular form in the Introduction to the Melbourne Catalogue, p. v. Prof. Gyldén's definitive corrections are—

$$\delta c = -0''\cdot16$$
$$\kappa = -0\cdot00314$$
$$\delta\beta = -0''\cdot783,$$

where δc is the correction to the adopted co-latitude, $(1+\kappa)R$ is the true refraction (R being the adopted value), and $\delta\beta$ the correction to the adopted horizontal flexure. A table of these corrections is given by Prof. Gyldén (*V. J. S.* Bd. xi. 184) from which the requisite quantities have been taken.

	N.P.D.	No. of Stars.	Upper - Lower. I.	II.	III.	Zenith Distance. Upper.	Lower.
	° ′		″	″	″	° ′	° ′
1	177 6	32	+0·44	−0·42	−0·82	49 16	55 4
2	173 7	35	+0·68	−0·20	−0·59	45 17	59 3
3	168 20	10	+0·14	−0·78	−1·13	40 30	63 50
4	162 39	7	−0·18	−1·21	−1·54	34 49	69 31
5	157 12	4	+0·61	−0·61	−0·93	29 22	75 58
6	152 38	5	+0·89	−0·65	−0·93	24 48	79 32
7	148 54	5	+1·43	−0·61		21 4	83 16
8	146 4½	3	+3·67	+0·70		18 14	86 5½

I. Results as printed.

II. „ with Stone's corrections.

III. „ with Gyldén's corrections.

If we now assume that

$\frac{x}{2}$ = correction to adopted co-latitude,
R = tabular refraction at upper transit,
R $(1-y)$ = true refraction at upper transit,
R_1 = tabular refraction at lower transit,
$R_1 (1-y)$ = true refraction at lower transit,

then our equations of condition are

$$x + y\,(R + R_1) = \text{N.P.D. upper} - \text{N.P.D. lower.}$$

In the last group (8) I now substitute for the mean refractions of Bessel's Supplementary Table those of the *Fundamenta* multiplied by the factor requisite to bring them up to the system of the *Tabulæ*, so that the refractions are then all found from the same system. The coefficients of y are computed with barometer and thermometer readings at their mean values for Melbourne; it would be more satisfactory to use the actual readings for the last two groups, but the want of the requisite detailed information renders this impossible.

The following are then the equations of condition:—

	Equations of Condition.	Weights.	Residuals.
1	$x + 148\,y = +0''.44$	149	$-0''.05$
2	$x + 153\,y = +0.68$	123	-0.27
3	$x + 165\,y = +0.14$	40	$+0.33$
4	$x + 192\,y = -0.18$	25	$+0.78$
5	$x + 242\,y = +0.61$	15	$+0.23$
6	$x + 326\,y = +0.89$	14	$+0.34$
7	$x + 473\,y = +1.43$	14	$+0.50$
8	$x + 724\,y = +4.38$	6	-1.26

Proceeding to solve these by the method of least squares, I find for the normal equations

$$386\,x + 71431\,y = 218.21$$
$$71431\,x + 16797189\,y = 57332.24,$$

whence

$$x = -0''.311 \pm 0''.203$$
$$y = +0.00474 \pm 0.00097.$$

If y be supposed to be without error, the probable error of x becomes $\pm 0''.092$. The sum of squares of residuals (the weights of course being taken into account) is 44.43.

By discussing all the available observations of circumpolar stars made during the years 1871–1875 in the above manner, we therefore find that the adopted co-latitude ought to be diminished by $0''.16$, and that Bessel's Refractions ought to be diminished by

0·00474 × Refraction. It will be remarked, however, that the residuals are not very satisfactory, and that the observations are affected by a considerable systematic error, not depending on refraction, in the neighbourhood of N.P.D. 165°.

For the purpose of comparison with the Greenwich Nine-Year Catalogue the Melbourne results, for stars which are contained in that catalogue, have been reduced to the epoch 1872·0; and mean places computed as in the case of circumpolar stars. The differences Greenwich *minus* Melbourne have then been found; the Greenwich places being corrected by the quantities given in the Addendum to the Introduction to the Nine-Year Catalogue, so that the refractions are Bessel's unaltered. Proceeding in the same manner as for the circumpolar stars, the results have been combined in groups, the weights for the Greenwich observations being found from the following expression for the probable error of a result

$$\epsilon^2 = (0\cdot26)^2 + \frac{e^2}{n},$$

where n is the number of observations, and e is taken from Mr. Stone's values of the probable errors of Greenwich observations at different zenith distances; whilst for the Melbourne observations the formula cited above (in the discussion of the circumpolar stars) has been used. Then for the combination we have

$$w = \frac{1}{\epsilon_G^2 + \epsilon_M^2}, \text{ and } W = \Sigma w.$$

The following table exhibits the results of this comparison:—

	N.P.D.	No. of Stars.	Greenwich − Melbourne. I.	II.	III.	Zenith Distance. Greenwich.	Melbourne.
	° ′		″	″	″	° ′	°
1	41 33	3	+3·72	−2·99	+0·72	3 2	86 13½
2	44 42	7	+2·61	−1·33	+0·23	6 11	83 8
3	47 27	8	+2·47	−0·37	+0·41	8 56	80 23
4	50 55	7	+1·22	−0·85	−0·53	12 24	76 55
5	57 20	6	+0·87	−0·47	−0·56	18 49	70 30
6	62 14	20	+1·08	+0·06	−0·19	23 43	65 36
7	67 31	13	+1·37	+0·59	+0·21	29 0	60 19
8	72 20	9	+1·27	+0·65	+0·21	33 49	55 30
9	76 59	16	+0·90	+0·40	−0·08	38 28	50 51
10	82 7	16	+0·56	+0·16	−0·34	43 36	45 53
11	87 24	16	+0·48	+0·18	−0·34	48 53	40 26
12	92 47	9	+0·01	−0·21	−0·73	54 16	35 3
13	98 25	11	+0·60	+0·45	−0·06	59 54	29 25
14	102 51	4	+0·52	+0·42	−0·06	64 20	24 59
15	107 9	11	+0·40	+0·35	−0·10	68 38	20 41
16	112 50	6	+0·06	+0·07	−0·36	74 19	15 0
17	117 1	8	+0·04	+0·09	−0·30	78 30	10 49
18	120 18	1	+0·96	+1·07	+0·67	81 47	7 32
19	124 9	1	−2·19	+1·99	+2·31	85 38	3 41

I. Greenwich corrected, Melbourne as printed.

II. „ „ „ with Stone's corrections.

III. „ „ „ with Gyldén's corrections.

Groups (18) and (19) containing only one star in each—viz. *Fomalhaut* and *α Columbæ* respectively—have not been used in the subsequent computation.

Now, if we assume

True refraction for Greenwich = tabular $(1-x) = R\ (1-x)$,

„ „ Melbourne = tabular $(1-y) = R_1\,(1-y)$,

Correction for relative error of adopted latitudes = δl,

then the equations of condition are

$$Rx + R_1 y + \delta l = \text{Greenwich} - \text{Melbourne}.$$

The discordances are of course taken from column I. of the table, except that in the case of (1) the result of substituting the mean refractions of the *Fundamenta* brought up to the system of the *Tabulæ* for those of the Supplementary Table has been used; also the Greenwich results have been corrected for assumed error of the thermometer (+0°·55). In computing R and R_1 the barometer and thermometer readings are taken at their mean values for the two Observatories. The equations of condition then become:—

	Equations of Condition.	Weights.	Residuals.
1	$3x + 723y + \delta l = +5''\cdot22$	6	$-0''\cdot50$
2	$6x + 444y + \delta l = +2\cdot61$	26	$+0\cdot42$
3	$9x + 325y + \delta l = +2\cdot48$	31	$-0\cdot22$
4	$13x + 241y + \delta l = +1\cdot23$	36	$+0\cdot50$
5	$20x + 160y + \delta l = +0\cdot89$	33	$+0\cdot32$
6	$25x + 126y + \delta l = +1\cdot10$	92	$-0\cdot10$
7	$32x + 101y + \delta l = +1\cdot41$	65	$-0\cdot56$
8	$39x + 83y + \delta l = +1\cdot31$	53	$-0\cdot57$
9	$46x + 70y + \delta l = +0\cdot95$	88	$-0\cdot28$
10	$55x + 59y + \delta l = +0\cdot62$	94	$-0\cdot01$
11	$66x + 49y + \delta l = +0\cdot55$	81	$+0\cdot01$
12	$80x + 40y + \delta l = +0\cdot09$	48	$+0\cdot42$
13	$100x + 32y + \delta l = +0\cdot71$	56	$-0\cdot22$
14	$120x + 27y + \delta l = +0\cdot65$	16	$-0\cdot16$
15	$147x + 22y + \delta l = +0\cdot56$	48	$-0\cdot07$
16	$203x + 15y + \delta l = +0\cdot27$	26	$+0\cdot25$
17	$277x + 11y + \delta l = +0\cdot34$	31	$+0\cdot25$

Proceeding to solve these by the method of least squares, I find

$$6636741\,x + 2629857\,y + 54905\,\delta l = 34428{\cdot}67$$
$$2629857\,x + 18102512\,y + 84176\,\delta l = 135055{\cdot}82;$$

whence

$$x = +0.00237 - 0{\cdot}00682\,\delta l$$
$$y = +0{\cdot}00711 - 0{\cdot}00366\,\delta l.$$

If

$$\delta l = +0{\cdot}''156 \pm 0{\cdot}''102,$$

as found from the observations of circumpolar stars (assuming that the Greenwich latitude requires no correction), then we have

$$x = +0{\cdot}00131 \pm 0{\cdot}00094$$
$$y = +0{\cdot}00654 \pm 0{\cdot}00077.$$

Therefore, by making use of all the available stars observed at Melbourne during the period 1871–1875, we find, for stars south of the zenith, that the adopted refractions should be diminished by

$$0{\cdot}00474 \times \text{Refraction};$$

whilst from the observations of stars north of the zenith it appears that the adopted refractions should be diminished by

$$0{\cdot}00654 \times \text{Refraction}.$$

It is evident that, so far, we have no reason to conclude that the constant of refraction is different in different directions; as of course, from the nature of the case, if we assume that the factors are, or may be, different, we are sure to get results differing to some extent. In fact, Mr. Stone found differences fully as great as these in his discussion of the Greenwich refractions from North and South stars.

It may be remarked in passing that the value of x found above indicates that if the correction had not been applied to the Greenwich thermometer readings, the resulting correction to the adopted refractions would have been practicably insensible. So that, as Mr. Christie has pointed out, as far as 80° Z.D. Bessel's Refractions accurately represent the Greenwich observations, when the thermometer reads about half a degree too high.

Suppose now that we reject all stars which pass the meridian at or below 85° Z.D. Then the normal equations derived from the observations of circumpolar stars become

$$380\,x + 67087\,y = 191{\cdot}93$$
$$67087\,x + 13652133\,y = 38305{\cdot}52.$$

From these equations I find

$$x = +0''.073 \quad \pm 0''.218$$
$$y = +0.00245 \pm 0.00115.$$

If y were without error, the probable error of x would be $\pm 0''.085$. The sum of squares of residuals, taking the weights into account, is 26·14.

We see, therefore, that the correction to the assumed co-latitude has changed sign, and the factor for correction to Bessel's Refractions is about half what it was before.

Proceeding in the same way with the North stars I find

$$6636687\,x + 2616843\,y + 54887\,\delta l = 34334{\cdot}71$$
$$2616843\,x + 14966138\,y + 79838\,\delta l = 112411{\cdot}46;$$

therefore

$$x = +0.00237 - 0.00662\,\delta l$$
$$y = +0.00710 - 0.00418\,\delta l.$$

The value of y derived from the comparison with Greenwich therefore remains the same as when all the stars were used, provided the same value of δl is substituted. If, however, we use the new value of δl, viz.

$$-0''.037 \pm 0''.109,$$

we find

$$y = +0.00725 \pm 0.00067.$$

The factors derived from North and South stars are now discordant to a considerable extent.

If we introduce into our normal equations only results deduced from stars passing the meridian above 82° Z.D. this discordance is still more strongly marked. Thus from the circumpolars we have

$$366\,x + 60465\,y = 171{\cdot}91$$
$$60465\,x + 10519927\,y = 28836{\cdot}06.$$

The solution of these equations gives

$$x = +0''.334 \quad \pm 0''.403$$
$$y = +0.00082 \pm 0.00221.$$

If y be supposed to be without error, the probable error of x becomes $\pm 0''.085$. The sum of squares of residuals is 22·87. The factor for correction of the adopted refractions is again much smaller than before; and, taking into account the magnitude of its probable error, practically unimportant.

For the North stars passing the meridian above 82° Z.D. we have

$$6635751\,x + 2547579\,y + 54731\,\delta l = 33927{\cdot}55$$
$$2547579\,x + 9840602\,y + 68294\,\delta l = 82281{\cdot}62;$$

whence

$$x = +0{\cdot}00211 - 0{\cdot}00642\,\delta l$$
$$y = +0{\cdot}00781 - 0{\cdot}00534\,\delta l;$$

or the value of y is still the same, if δl is the same. If

$$\delta l = -0''{\cdot}167 \pm 0''{\cdot}202$$

as found from circumpolar stars above 82° Z.D. at lower transit, then

$$y = +0{\cdot}00876 \pm 0{\cdot}00134.$$

The discordance between the values of y North and South is therefore much increased.

If we proceed a step further and reject all stars below 77° Z.D. we find from the circumpolar stars actually a negative value for y; whilst for the North stars, compared with Greenwich the value still remains practically unaltered.

It appears, therefore, as the general result of this investigation, that when the very low stars are included, where of course the refraction comes in with greatest effect, we get a fair agreement between the corrections derived from North and from South stars; but as we go on omitting the lower stars, the discordance between the corresponding corrections on the North and South sides increases rapidly. It is evident, then, that there is a serious systematic error affecting the observations made on one side or other of the zenith. There is, however, no reason to suppose that this arises from a difference in the mean refractions, resulting from the circumstance of the meridian on one side being over the sea and on the other side over the land, and the atmosphere therefore being in a different state with regard to moisture, as has been supposed; for, *if this* were the true explanation, the discordance referred to ought at all events not to *diminish* as the lower stars are included, whereas I have shown that it diminishes rapidly until at last it becomes practically insignificant. The origin of the discordance is more probably some mechanical defect in the instrument, as Mr. Christie has suggested.

I may remark that in Mr. Stone's discussion of the Melbourne results 1863–1865 referred to above, all the stars used are above 82° Z.D., so that the discordance which he has found is borne out to a great extent by the results of this investigation.

It becomes now a question of interest to determine, as far as possible, on which side of the zenith the Melbourne N.P.D.'s are most correct. Although no great certainty can be arrived at

with the means of judging the question which are available to us, still I think it most probable that it is the stars on the north side of the zenith which are most affected by systematic error. For it will be remarked that the circumpolar stars give very much the same results as Main's exhaustive discussion gave for the Greenwich N.P.D.'s—that is, that Bessel's Refractions unaltered answer very well down to 82° Z.D.; from 82° to 85° Z.D. that the refractions of the *Tabulæ* appeared to require to be diminished by ·00230 × Refraction; and below 85° Z.D. that the refractions of the *Fundamenta* should be used diminished by ·00152 × Refraction, the refractions of the *Fundamenta* being equivalent to those of the *Tabulæ* diminished by ·00327 × Refraction of the *Tabulæ*. These results agree very closely with those found in this paper for the Melbourne circumpolar stars, and therefore I conclude that it is probable that the N.P.D.'s of these stars are substantially correct. And in fact the substitution of Main's corrections below 82° Z.D. gives smaller residuals than I have obtained from the solution by the method of least squares. For, applying these corrections and putting $y=0$, we get

$$x = +0''.474 \pm 0''.065,$$

and the sum of squares of residuals (the weights being taken into account) is only 24·96: in the former case it was 44·43.

If we attempt to determine the co-latitude from observations of circumpolar stars which pass the meridian above 82° Z.D. at the lower transit, assuming that the refractions of the *Tabulæ* are correct down to that limit, we find

$$x = +0''.470 \pm 0''.077,$$

and the sum of squares of residuals is 23·54: in the solution given above for these stars this quantity comes out 22·87, so that our assumption is satisfactorily borne out, considering the difficulty of separating errors of co-latitude and of refraction at the latitude of Melbourne. I should recommend therefore that, for the present, a correction of $+0''.20$ be applied to the co-latitude assumed in this volume, thus making

$$\text{Co-latitude} = 52^\circ\ 10'\ 6''.8;$$

and that the refractions of Bessel's *Tabulæ* be used unaltered, at all events down to 82° Z.D.

I understand that efforts are being made to obtain a new Transit Circle, more suited to the requirements of modern astronomy, for the Melbourne Observatory. Until we have a series of observations made with such an instrument, I do not think that we can approximate more closely either to the true value of the latitude, or to the value of the constant of refraction

required by the Melbourne observations, than I have done in the present paper.

I add here, for convenience of reference, the separate comparisons, both for circumpolars and for stars compared with the Greenwich Catalogue, of all stars which pass the meridian of Melbourne below 82° Z.D. observed in the years 1871–1875.

I. *Circumpolar Stars.*

Star's Name.	N.P.D.	Upper – Lower.	Number of Obs. Upper.	Lower.
	° ′	″		
β Centauri	149 46	−0·06	7	5
ε Argûs	149 6	+2·18	7	6
η Argûs	149 1	+1·86	12	4
ι Argûs	148 45	+1·96	8	7
α Eridani	147 53	+1·14	6	11
α Pavonis	147 8	+3·19	10	9
ζ Aræ	145 47	+3·38	6	
α Doradûs	145 18	+5·10	7	5

II. *North Stars.*

Star' Name.	N.P.D.	Greenwich – Melbourne.	Number of Obs. Greenwich.	Melbourne.
	° ′	″		
δ Cygni	45 11	+1·91	19	5
α Cygni	45 11	+1·95	48	13
ξ Andromedæ	45 8	+5·28	3	3
β Aurigæ	45 4	+0·77	16	6
ψ Ursæ Majoris	44 48	+1·11	13	6
α Aurigæ	44 8	+3·12	45	12
τ Herculis	43 23	+0·46	23	2
δ Persei	42 37	+2·55	5	5
51 Andromedæ	42 1	(+10·50)	6	3
ι Ursæ Majoris	41 27	+3·04	17	10
α Persei	40 36	+6·73	37	7
η Ursæ Majoris	40 3	(+12·81)	77	4

The quantities enclosed within brackets have not been used in the present investigation. The above comparison is made with the Greenwich places as printed in the Nine-Year Catalogue.

Transit Times of the Spots on Jupiter. By W. F. Denning, Esq.

During the past autumn and winter a number of observations of *Jupiter* were made here* for the purpose of estimating the times of transit of the Red Spot and of other conspicuous objects on his disk. The telescope was a Browning-With Reflector of 10 inches' aperture, power 300, and a Barlow lens, which increased it to 450, was invariably used. The results are given in a tabulated form for each object separately.

Transits of the Great Red Spot.

		Preceding End.	Middle.	Following End.	Middle of Spot. Precedes first Meridian.
		h m	h m	h m	m
1880, Sept.	27	10 26	10 51	11 15	54·9
	30	7 56	8 16	8 39	57·4
Oct.	11	11 47	12 16	12 46	52·1
	14	9 17	9 46	10 12	49·7
	18	12 39	13 5	13 35	44·5
	23	11 44	12 12	12 41	42·1
	24	7 34	7 59	8 26	45·9
	29	6 40	7 8	7 36	41·7
Nov.	2	10 0	10 26	10 57	37·8
	3	5 44	6 12		42·7
	4	11 36	12 4	12 38	36·9
	8	4 50	5 21	5 53	38·8
	9	10 41	11 12	11 39	34·1
	14		10 20	10 45	31·4
	17	7 17	7 44	8 12	35·6
	19	8 59	9 23	9 52	33·9
	20	4 48	5 14	5 43	33·8
	21	10 38	11 4	11 32	30·3
	25		4 25	4 49	28·6
	27	5 34	6 0	6 29	31·0
	28	11 24	11 49		28·5
	29	7 11	7 40	8 9	28·4
Dec.	2	4 38	5 8	5 37	28·9
	10	11 20	11 47		20·2

* An abstract of my earlier observations appears in the *Monthly Notices* for November, 1880, pp. 44–46.

	Preceding End.	Middle.	Following End.	Middle of Spot. Precedes first Meridian.
	h m	h m	h m	m
1880, Dec. 15	10 30	10 56	11 22	17·5
20	9 38	10 4	10 33	16·0
25	8 40	9 12	9 39	14·6
28	6 15	6 40	7 7	15·5
30	7 50	8 21	8 50	12·4
31		4 11	4 39	13·5
1881, Jan. 6	8 40	9 7	9 33	11·1
7	4 31	4 57	5 25	12·3
12		4 5	4 32	11·3
13	9 27			
14	5 17	5 44	6 13	10·3
16	6 54	7 23	7 52	9·2
31		4 49	5 18	4·9
March 17	6 36	7 1		+3·5

Average duration of transit=55·5 min. (28 transits).

On December 2, 1880, the following end of the spot had the same longitude as the assumed first meridian of *Jupiter* computed by Mr. Marth* from the rotation period of spots seen in 1834–5 and 1862—namely, $9^h\ 55^m\ 27^s{\cdot}08$, which corresponds to a daily rate of 870°·600. The red spot moves slower than this, its period being as nearly as possible 7 seconds greater than that adopted in calculating the passages of the first meridian. Thus during the period embraced by the above observations the comparative times of transit of the spot and of the assumed first meridian shows a considerable difference, for while on Sept. 27, 1880, the middle of the spot preceded this element by 55 minutes, it had so far lost ground by March 17, 1881, as to follow it by $3\frac{1}{2}$ minutes; the difference amounting to nearly an hour in the 171 days. The centre of the spot must have had coincident longitude with the assumed first meridian on about February 26, but no observations could be made at that epoch owing to cloudy weather.

In October I frequently noticed a dusky elongated marking or short streak south preceding the red spot about $1\frac{1}{2}$ hour. It crossed the central meridian of *Jupiter* at about $10^h\ 35^m$ on Oct. 11 and at the end of that month I began to note it particularly. The following are the times when the centre or following end was estimated as central :—

* See *English Mechanic*, Nos. 807, 814. and 821.

Transits of Dusky Streak s.p. Red Spot.

	Middle.	Following End.	Middle Precedes ♑'s First Meridian.	Precedes Red Spot.
	h m	h m	h m	h m
1880, Oct. 11	10 35?			1 41?
31	7 15		2 11·8	
Nov. 2	8 48	8 59	2 15·8	1 38
4	10 28	10 37	2 12·9	1 36
8	13 41	13 53	2 14·2	
12	6 59		2 15·2	
17	6 3	6 15	2 16·6	1 41
22	5 7		2 18·2	
24	6 46		2 16·6	
26	8 25		2 15·0	
27	4 16		2 15·0	1 44
28	10 1	10 16	2 16·5	1 48
29	5 48	6 5	2 20·4	1 52
Dec. 18		6 38		
20	7 56	8 12	2 24·0	2 8
21		4 11		
25	6 59	7 14	2 27·6	2 13
28		4 44		
30	6 3	6 20	2 30·4	2 18
1881, Jan. 6	6 45	6 59	2 33·1	2 22
8	8 22	8 40	2 34·0	
13	7 24	7 43	2 39·1	
16	4 52	5 8	2 40·2	2 31
Feb. 16		5 35		

The first observation on Oct. 11 is doubtful; the time is probably too early.

Average duration of transit (computed from 13 observed passages of the middle and following end of the spot) 29·4 min., so that the spot was slightly more than half the length of the red spot. When definition was poor it appeared more diffused and longer than when the image was sharp and the spot seen in a favourable and true aspect. Mr. Barnard, at Nashville, Tennessee, had figured this spot (*English Mechanic*, No. 804) as early as July 25, 1880, when it was apparently attached to the preceding end of the red spot; but it had subsequently been rotating in shorter intervals than that object, and its swifter motion had caused its gradual drift westwards. In the above table I have compared the period as observed here by which

it preceded *Jupiter's* first meridian and the red spot, and it will be noticed that while on November 2 and 4 it came to transit slightly more than 1½ hour before the red spot, the interval had increased to 2½ hours by January 16. In fine, its period of rotation was performed in 17½ seconds less time than the red spot,* and this accords exactly with similar observations by Mr. Barnard, who has been watching another object of the same character in the southern hemisphere of *Jupiter*, and deduced a period of $9^h\ 55^m\ 16^s{\cdot}176$.† There were a number of these dusky short streaks visible in this region of the planet during his recent opposition; they were markedly persistent, and, though preserving the same positions relatively to each other, they all moved quicker than the red spot.

On September 18, 1880, I detected a very brilliant spot just south of the Jovian equator, and the same object was observed on many subsequent occasions. After the middle of October I obtained as many of its transits as possible, for it gave the most unmistakeable signs of great proper motion. The same feature was frequently observed by Schmidt at Athens in October and November 1880 (see *Astronomische Nachrichten*, No. 2353), and he has deduced its rotation period as $9^h\ 50^m$, or $5^m\ 34^s$ less than the red spot.

The following are the transits observed at Bristol:—

Bright Spot South of Equator.

	h m		h m
1880, Sept. 18	11 50?	1880, Nov. 29	5 27
Oct. 20	11 38	Dec. 3	7 54
30	7 28	10	7 14
Nov. 2	14 15	12	8 27
3	9 47	15	5 16
4	5 25	21	8 49
8	7 48	23	10 4
17	8 14	28	7 59
19	9 23	1881, Jan. 6	8 27
20	5 2	7	4 5
21	10 41	13	7 37
26	8 44	16	4 28
27	4 19	Feb. 23	7 5?
28	9 51		

* See *English Mechanic*, No. 824, Jan. 7, 1881, p. 421.

† The small spot referred to was mentioned in *Monthly Notices*, Nov. 1880, p. 46, as "maintaining a permanent position with reference to the red spot," but this was stated before my prolonged observations had enabled me to detect the differences here referred to.

I gave the period as $9^h\ 50^m\ 5^s$ in *English Mechanic*, No. 818, Nov. 26, p. 277,* and this agrees with that derived by Mr. Marth (*ibid.* No. 830, Feb. 18, p. 564) from more comprehensive observations during the interval Sept. 18, 1880–Jan. 13, 1881, by Schmidt, Corder, Dennett, McCance, Barnard, and myself, including 55 transits which indicated the daily rate of rotation as 878°·48, and the corresponding period $9^h\ 50^m\ 6^s{\cdot}6$. This remarkable spot was noticed by many observers independently. It came to conjunction with the red spot on Nov. 19 and Jan. 3, but on the latter date clouds prevented observation. After Jan. 16, a prolonged period of overcast skies prevented a continuance of the observations, and the spot was not identified afterwards, though on Feb. 23, at $7^h\ 5^m$ I saw a bright object nearly in the calculated place, and it was watched in transit. Its absolute identity with the spot so frequently seen during Oct., Nov., and Dec., could not, however, be inferred, and there is also some doubt attached to the original observation on Sept. 18, owing to the long intervening period between that date and the next observed transit on Oct. 20. There were several of these bright spots presented on *Jupiter* in very nearly the same latitude, so that, unless the observations were made at short intervals, there was considerable difficulty in distinguishing them apart. But, fortunately, in the case of the brilliant object I have been particularly alluding to, it was of special type, being considerably brighter and more conspicuous than any others of its class, and situated slightly further south. It was occasionally noted as extremely faint, while at other times it seemed very distinct and comparable to the bright disk of a satellite on the limb of *Jupiter*, near the beginning or end of a transit. Generally, however, it was very distinct, and its rapid motion relatively to the red spot could be clearly traced on succeeding nights. It was preceded and followed closely by spots of similar character, though fainter, and intermittently visible, which showed the same swift movement westwards; indeed, the mass of detail manifested on the equator appeared to be influenced by the same rapid current relatively to the red spot, and by some singular variations in form. Many large white patches made their appearance, but soon became diffused along the light N. border of the great South belt, and there were dusky strips intervening which also participated in the remarkable motion and variations referred to.

The small dark spots visible in a belt N. of the great equatorial zone, which I referred to in my paper in *Monthly Notices* for Nov. 1880, pp. 44–46, were re-observed as frequently as possible, but their variations of position and form were so great as to render it almost impossible to trace the identical objects at their succeeding apparitions. The two chief spots of the cluster were, however, followed as long as practicable, and the following transits observed here:—

* See also *Monthly Notices* for Nov. 1880, p. 45.

		I.	II.			I.	II.
		h m	h m			h m	h m
1880, Oct.	29	7 31	7 54	1880, Dec.	2	4 55	5 39
Nov.	2	9 31	9 52		15		6 48
	3	5 5	5 31		20		4 11
	4	10 41	11 4	1881, Jan.	6	7 30	8 11
	8	12 37	13 3		7		3 50
	23	5 14	5 39		8	8 46	
	29		8 44				

These spots travelled with greater velocity than any other of the markings. The approximate period was $9^h\ 48^m$, but the motion was not uniform. They were observed to overtake and pass the white spot on the equator early in January.

Towards the middle of January they apparently formed a complete girdle around the planet, but the spots were evidently losing their distinctive character. What had formerly been a single belt N. of the great equatorial zone was now a double belt; the one nearest the equator was the most conspicuous, and had evidently been formed by the dispersion of the materials of the spots, which had been going on under the eyes of observers during the three preceding months.

Ephemeris for Physical Observations of Jupiter, 1881–82. By A. Marth, Esq.

Greenwich Noon.	Angle of Position of ♃'s Axis.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	Annual Parallax A−L.	O−L.	Longitude of ♃'s Central Meridian.	Diff.	Corr. for Phase.
	°	°	°	°	°	°	°	°
1881, June 28	343·087	+3·058	+3·050	−8·929	87·612	96·05	4351·44	+0·35
July 3	343·405	3·076	3·053	9·423	86 662	127·49	1·50	·39
8	343·714	3·093	3·055	9·881	85·749	158·99	1·56	·43
13	344·013	3·110	3·057	10·297	84·878	190·55	1·61	·46
18	344·301	3·127	3·059	10·667	84·053	222·16	1·67	·50
23	344·575	3·143	3·061	10·989	83·277	253·83	1·73	·53
28	344·834	+3·159	+3·063	−11·259	82·553	285·56	1·80	+·55
Aug. 2	345·075	3·175	3·065	11·473	81·885	317·36	1·87	·57
7	345·297	3·190	3·066	11·628	81·276	349·23	1·93	·59
12	345·498	3·206	3·067	11·720	80·730	21·16	2 00	·60
17	345·675	3·221	3·068	11·745	80·251	53·16	2·07	·60
22	345·828	3·235	3·069	11·700	79·842	85·23	2·15	·59
27	345·954	+3·249	+3·070	−11·581	79·507	117·38	2·22	+·58
Sept. 1	346·051	3·263	3·070	11·385	79·249	149·60	2·28	·56
6	346·119	3·276	3·070	11·110	79·071	181·88	2·36	·54
11	346·157	3·289	3·070	10·754	78·974	214·24	2·43	·50
16	346·163	3·301	3·070	10·315	78·961	246·67	2·50	·46
21	346·137	3·311	3·069	9·791	79·032	279·17	2·57	·42

Greenwich Noon.	Angle of Position of ♃'s Axis.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	Annual Parallax A−L.	O−L.	Longitude of ♃'s Central Meridian.	Diff.	Corr. for Phase.
	°	°	°	°	°	°	°	°
1881, Sept. 26	346·080	+3·320	+3·069	−9·184	79·186	311·74	2·63	+·37
Oct. 1	345·992	3·327	3·068	8·495	79·423	344·37	2·68	·31
6	345·874	3·333	3·067	7·726	79·740	17·05	2·73	·26
11	345·729	3·337	3·066	6·882	80·132	49·78	2·77	·21
16	345·559	3·338	3·065	5·968	80·595	82·55	2·80	·16
21	345·366	3·337	3·063	4·990	81·122	115·35	2 82	·11
26	345·155	+3·333	+3·061	−3·958	81·702	148·17	2·83	+·07
31	344·930	3·326	3·059	2·884	82·325	181·00	2·83	·04
Nov. 5	344·696	3·317	3·057	1·778	82·980	213·83	2·81	+·01
10	344·456	3·304	3·055	−0·652	83·655	246·64	2·77	·00
15	344·216	3·289	3·053	+0·482	84·338	279·41	2·73	·00
20	343·981	3·271	3·050	1·608	85·015	312·14	2·67	−·01
25	343·754	+3·250	+3·047	+2·714	85·672	344·81	2·60	−·03
30	343·541	3·228	3·044	3·787	86·296	17·41	2·52	·06
Dec. 5	343·345	3·204	3·041	4·817	86·875	49·93	2·43	·10
10	343·169	3·179	3·037	5·792	87·400	82·36	2·33	·15
15	343·015	3·152	3·034	6·702	87·862	114·69	2·23	·20
20	342·886	3·125	3·030	7·541	88·252	146·92	2·12	·25

Greenwich Noon.	Angle of Position of ♃'s Axis.	Latitude of Earth above ♃'s Equator.	Latitude of Sun above ♃'s Equator.	Annual Parallax A--L.	O−L.	Longitude of ♃'s Central Meridian.	Diff.	Corr. for Phase.
	°	°	°	°	°	°	°	°
1881, Dec. 25	342·782	+ 3·099	+ 3·026	+ 8·302	88·564	179·04	2·02	− ·30
30	342·705	3·073	3·022	8·980	88·793	211·06	1·91	·35
1882, Jan. 4	342·656	3·047	3·018	9·573	88·938	242·97	1·80	·40
9	342·635	3·022	3·013	10·079	88·997	274·77	1·70	·44
14	342·642	2·998	3·008	10·499	88·968	306·47	1·61	·48
19	342·677	2·976	3·003	10·832	88·853	338·08	1·51	·51
24	342·740	+ 2·955	+ 2·998	+ 11·080	88·655	9·59	1·42	− ·53
29	342·830	2·936	2·993	11·246	88·375	41·01	1·33	·55
Feb. 3	342·946	2·918	2·988	11·333	88·015	72·34	1·26	·56
8	343·087	2·901	2·982	11·344	87·580	103·60	1·19	·56
13	343·253	2·885	2·976	11·284	87·073	134·79	1·12	·55
18	343·444	2·871	2·970	11·156	86·498	165·91	1·07	·54
23	343·659	+ 2·858	+ 2·964	+ 10·963	85·859	196·98	1·02	− ·52
28	343·897	2·847	2·958	10·710	85·160	228·00	0·97	·50
March 5	344·156	2·836	2·952	10·401	84·406	258·97	0·93	·47
10	344·436	2·826	2·945	10·042	83·602	289·90	0·89	·44
15	344·737	2·817	2·938	9·635	82·750	320·79	0·87	·40
20	345·058	2·809	2·931	9·183	81·854	351·66	4350·85	·37
25	345·398	+ 2·802	+ 2·924	+ 8·691	80·918	22·51		−0·33

A − L is the difference of the Jovicentric longitudes of the Sun and the Earth, reckoned in the plane of *Jupiter's* equator, O − L the difference of longitudes of *Jupiter's* vernal equinoctial point O and of the point L of his celestial equator, which is in opposition to the Earth. If the "Corr. for phase" in the last column is applied to the "Longitude of ♃'s central meridian," or of the meridian directed to the Earth, the longitude of the meridian is found which bisects the illuminated disk of *Jupiter*.

The daily rate of rotation adopted in the present ephemeris is 870°·42, the corresponding period being $9^h\ 55^m\ 34^s{\cdot}47$. This rate represents very nearly the average motion of the great reddish spot between the last months of 1878 and the first months of 1881. The observations show that during this interval the motion of the spot has slackened, and the rate of retardation seems to be increasing. In the ephemerides, published in the *Monthly Notices*, for the last six apparitions of *Jupiter*, the adopted First Meridian was assumed to rotate at the daily rate 870°·60. While at the time of the early observations in 1878 the middle of the great spot preceded the First Meridian by nearly 160°, this interval continually decreased by the difference of their motions, and towards the end of February 1881 the First Meridian has overtaken the middle of the spot. On account of the obvious advantage of keeping it conveniently near the middle, it appeared expedient to introduce the altered rate of rotation and to fix upon Feb. 28·0 as the time when the computed longitudes of the central meridian should be the same in the altered system as in the former one, so that the correction—

$$+0^\circ{\cdot}18\ (1881,\ \text{Feb.}\ 18{\cdot}0 - t)$$

must be applied to the longitudes of the former ephemerides, in order to reduce them to the system adopted in the present ephemeris. This correction is for

1875, Feb. 20	+	36°·0	1877, April 30	+	252°·0	1879, July 9	+ 108°·0
May 31		18·0	Aug. 8		234·0	Oct. 17	90·0
1876, May 15		315·0	Nov. 16		216·0	1880, Jan. 25	72·0
July 4		306·0	1878, June 4		180·0	Aug. 12	36·0
Aug. 23		297·0	Sept. 12		162·0	Nov. 20	18·0
Oct. 12	+	288·0	Dec. 21	+	144·0	1881, Feb. 18	+ 0·0

The corrected longitudes corresponding to the recorded times of the observed passages of the great spot across the middle meridian may then serve as the foundation for a thorough investigation of its motions; but I defer the publication of a list of observed passages and their corresponding longitudes till it can

be given with some completeness. A list of the sketches of *Jupiter* in possession of the Royal Astronomical Society, arranged separately for each apparition of the planet, according to the longitudes of their middle meridian, must also be deferred. For the present, I content myself with supplementing Prof. Pritchett's communication at the end of this paper, containing his observations of the passages of the red spot, made during the last apparition of *Jupiter* at the Morrison Observatory, Glasgow, Missouri, by adding the corresponding longitudes. The evidence on the motion of the spot, furnished by the partial lists of observed passages, published in the *Monthly Notices*, vol. xl. pp. 420–428, and p. 496, and in the *English Mechanic* of Oct. 29, Dec. 17, and Feb. 18 last, or vol. xxxii. pp. 182, 350, and 563, may be rendered available by adding to the "Corr. Longitudes" there given under the heading "Rot. 870°·43" the quantity

1878, Sept. 12	+ 94°·0	1880, Jan. 25	+ 89°·0
Dec. 21	93·0	Aug. 12	87·0
1879, July 9	91·0	Nov. 20	86·0
Oct. 17	+ 90·0	1881, Feb. 28	+ 85·0

which will reduce them to the system of the present ephemeris.

The only passages, observed in 1878, which have yet become publicly known, are those of Mr. Trouvelot. The observations were kindly communicated to me by letter, and the times, published on p. 496 of vol. xl., are in accordance with the letter, excepting the hour on Dec. 16, which should be 4 instead of 6, the "corresponding longitude" belonging to the correct time $4^h\,35^m$. Mr. Trouvelot has now published a general account of his "Observations on *Jupiter*" in the "Proceedings of the American Academy of Arts and Sciences," and a copy of his interesting paper has reached me just in time for the present paper. In the list of observed passages of the great spot on page 307, the time of passage

		h	m	h	m
1878, Sept. 30 is given	6	5	instead of	6	35 in the letter.
Oct. 2	7	5		7	45
Dec. 16	4	40		4	35

and, moreover, the time of passage of the centre on Sept. 25 is recorded. But though on page 308, where the discrepancy is pointed out, it is expressly stated that the time recorded in the paper is right, it is a question whether the correction for Oct. 2 is not in error. I now give the list of observed passages of 1878,

together with the corresponding longitudes of the middle meridian according to the system adopted in the present ephemeris.

	Observed Passages. Mean time Cambridge, Mass.			Corresponding Longitudes.		
	Prec. End.	Middle.	Foll. End.	Prec. End.	Middle.	Foll. End.
	h m	h m	h m	°	°	°
1878, Sept. 25		6 55			0·6	
30		6 5			2·0	
Oct. 2		? { 7 5 / 7 45			338·9 / 3·1	?
Nov. 12			6 50			10·2
24	6 10			348 4		
29	5 15			346·1		
Dec. 16		4 40			358·0	
23		5 33			1·2	
28		4 45			3·0	

The following table gives the values of the Equatoreal Diameter of *Jupiter* and "Def." its defect of illumination, of the Polar Diameter, the defect of which is insensible, and of the differences of limbs in right ascension and declination, with their defects of illumination, together with the auxiliary angles d and w, required in the computation, as explained in the "Addition" to last year's ephemeris, vol. xl. p. 490 ff.:—

Greenwich Noon.	Equatoreal Diameter.	Def. prec.	Polar Diameter.	Difference between Limbs in A.R.	Def. prec. l.	in Decl.	Def. south l.	d.	w.
	″	″	″	s	s	′	″	°	°
1881, June 28	35·10	0·21	32·87	2·423	0·014	33·06	0·02	8·91	269·80
July 3	35·47	·24	33·22	2·453	·015	33·41	·02	9·40	269·88
8	35·87	·27	33·60	2·484	017	33·78	·02	9·86	269·95
13	36·30	·29	34·00	2·517	·019	34·18	·02	10·28	270·01
18	36·76	·32	34·43	2·552	·021	34·61	·02	10·65	270·07
23	37·25	·34	34·89	2·589	·022	35·06	·02	10·97	270·13
28	37·77	·36	35·37	2·627	·024	35·54	·03	11·24	270·19
Aug. 2	38·31	·38	35·88	2·668	·026	36·04	·03	11·46	270·25
7	38·88	·40	36·41	2·710	·026	36·57	·03	11·61	270·31
12	39·47	·41	36·96	2·753	·027	37·12	·03	11·70	270·37
17	40·08	·42	37·54	2·798	·028	37·70	·03	11·73	270·44
22	40·72	·42	38·13	2·844	·028	38·29	·02	11·68	270·52
27	41·37	·42	38·74	2·891	·028	38·90	·02	11·56	270·60
Sept. 1	42·03	·41	39·36	2·938	·027	39·52	·02	11·37	270·70
6	42·70	·40	39·99	2·986	·027	40·15	·02	11·09	270·81
11	43·38	·38	40·62	3·033	·025	40·79	·02	10·74	270·93
16	44·05	·35	41·25	3·080	·024	41·42	·02	10·30	271·06
21	44·71	·32	41·87	3·126	·022	42·04	·02	9·78	271·21
26	45·35	·29	42·47	3·170	·019	42·64	·01	9·17	271·40

Greenwich Noon.	Equatoreal Diameter.	Def. prec.	Polar Diameter.	Difference between Limbs in A.R.	Def. prec. l.	in Decl.	Def. south l.	d.	w.
	"	"	"	s	s	"	"	°	°
1881, Oct. 1	45·97	·25	43·05	3·211	·017	43·22	·01	8·48	271·62
6	46·54	·21	43·59	3·250	·014	43·77	·01	7·72	271·87
11	47·08	·17	44·09	3·285	·011	44·28	·01	6·88	272·19
16	47·55	·13	44·54	3·316	·009	44·73	·01	5·96	272·62
21	47·96	·09	44·92	3·342	·006	45·12	·00	4·99	273·21
26	48·30	·06	45·24	3·362	·004	45·44		3·96	274·09
31	48·56	·03	45·48	3·376	·002	45·69		2·89	275·58
Nov. 5	48·72	·01	45·63	3·385	·001	45·85		1·80	278·84
10	48·80	·01	45·70	3·387	·000	45·93		0·70	292·24
15	48·78	foll. l.	45·68	3·382	foll. l.	45·92		0·54	62·26
20	48·66	·01	45·57	3·370	·001	45·82		1·62	81·69
25	48·45	·03	45·38	3·352	·002	45·63	north l.	2·72	85·50
30	48·16	·05	45·10	3·329	·003	45·35	·00	3·79	87·14
Dec. 5	47·78	·08	44·75	3·300	·005	45·00	·01	4·81	88·07
10	47·33	·12	44·33	3·267	·008	44·58	·01	5·78	88·70
15	46·81	·16	43·84	3·229	·010	44·10	·02	6·69	89·11
20	46·24	·20	43·30	3·188	·013	43·56	·02	7·53	89·43
25	45·62	·24	42·72	3·144	·015	42·98	·02	8·29	89·70
30	44·97	·27	42·11	3·098	·017	42·37	·03	8·97	89·92

Greenwich Noon.	Equatoreal Diameter.	Def. prec. l.	Polar Diameter.	Difference between Limbs in A.R.	Def. prec. l.	in Decl.	Def. south l.	d.	w.
	″	′	″	s	s	″	″	°	°
1882, Jan. 4	44·29	·31	41·47	3·051	·019	41·73	·03	9·56	90·09
9	43·59	·34	40·82	3·003	·021	41·07	·03	10·06	90·23
14	42·89	·36	40·16	2·955	·023	40·41	·03	10·48	90·35
19	42·18	·38	39·50	2·907	·024	39·75	·03	10·81	90·46
24	41·48	·39	38·85	2·860	·025	39·09	·03	11·06	90·55
29	40·79	·39	38·20	2·814	·025	38·44	·03	11·23	90·63
Feb. 3	40·12	·39	37·57	2·770	·025	37·80	·03	11·32	90·69
8	39·47	·38	36·96	2·727	·025	37·18	·03	11·33	90·75
13	38·84	·37	36·37	2·686	·024	36·58	·03	11·27	90·80
18	38·23	·36	35·80	2·646	·023	36·00	·03	11·14	90·85
23	37·65	·34	35·26	2·609	·022	35·45	·03	10·95	90·89
28	37·10	·32	34·74	2·574	·021	34·93	·02	10·70	90·93
Mar. 5	36·58	·30	34·25	2·541	·020	34·43	·02	10·39	90·96
10	36·09	·28	33·79	2·511	·018	33·96	·02	10·03	90·99
15	35·63	·25	33·36	2·482	·017	33·53	·02	9·62	91·03
20	35·20	·23	32·96	2·456	·015	33·12	·01	9·17	91·06
25	34·80	0·20	32·59	2·432	0·013	32·73	0·01	8·68	91·09

The inclinations γ and the ascending nodes Γ of the orbits of the four satellites in reference to the plane of *Jupiter's* equator are the following, the nodes being reckoned from O, the point of the vernal equinox of *Jupiter's* northern hemisphere:—

1881	Sat. I. γ_1	Sat. I. Γ_1	Sat. II. γ_2	Sat. II. Γ_2	Sat. III. γ_3	Sat. III. Γ_3	Sat. IV. γ_4	Sat. IV. Γ_4
	°	°	°	°	°	°	°	°
June 8	0·0113	351·6	0·4736	357·53	0·1776	263·22	0·3281	330·42
Aug. 7	·0113	350·4	·4744	355·60	·1763	262·81	·3276	330·56
Oct. 6	·0114	349·2	·4752	353·67	·1749	262·38	·3270	330·68
Dec. 5	·0114	348·0	·4760	351·75	·1736	261·93	·3263	330·80
1882								
Feb. 3	·0113	346·7	·4766	349 82	·1723	261·47	·3256	330·90
April 4	0·0113	345·3	0·4772	347·89	0·1711	260·98	0·3248	330·98

If these values of Γ are added to the elongations O−L of the point O from superior conjunction, given in the first table, the angles Γ+O−L are the elongations of the ascending nodes of the orbits from superior conjunction. Hence the latitudes of the satellites above the plane of *Jupiter's* equator are easily found, if their elongations are known.

The following is Prof. Pritchett's communication:—

"Transits of Red Spot across the apparent central Meridian of *Jupiter*. Observations made at Morrison Observatory, between May 25, 1880, and Feb. 22, 1881, by C. M. Pritchett," to which I have added the corresponding longitudes of the middle meridian according to the system of longitudes adopted in the present ephemeris. The times are already reduced Greenwich mean times. The weights are on a scale from 1 to 5.

	Observed Passages. Prec. End.	Centre.	Foll. End.	Wt.	Corresponding Longitudes. Prec. End.	Centre.	Foll. End.
1880	h m	h m	h m		°	°	°
May 25		23 12·1		3		356·7	
June 6	22 34·6	23 5·6		3	337·8	356·5	
28			21 32·3	3			7·9
July 10	20 25·7	20 59·7	21 31·8	5	332·4	352·9	12·3
12		22 40·5	23 13·7	4		354·7	14·7
19	22 48·3	23 21·8		3	332·3	352·6	
22	20 18·9	20 52·4	21 26·3	4	333·3	353·6	14·1
24		22 29·3	23 1·3	3		353·1	12·4
29	21 5·8	21 36·8	22 6·5	4	334·8	353·6	11·5
31	22 39·8	23 13·3	23 45·8	4	332·6	352·8	12·5
Aug. 10	20 58·8	21 29·8	22 1·8	4	336·2	354·9	14·3
12	22 34·3	23 5·5	23 37·6	4	335·0	353·9	13·3
17		22 12·0	22 43·1	4		354·0	12·8
18	17 27·8	18 0·1	18 30·3	5	332·7	352·8	10·5

	Observed Passages. Prec. End.	Centre.	Foll. End.	Wt.	Corresponding Longitudes. Prec. End.	Centre.	Foll. End.
1880.	h m	h m	h m		°	°	°
23	16 32·2	17 3·8	17 34·3	4	331·5	350·7	9·2
30	17 20·3	17 52·8	18 23·3	4	334·4	354·0	12·5
Sept. 3		21 12·8	21 44·7	3		357·1	16·4
4	16 26·8	16 58·8	17 29·8	5	334·7	354·1	12·8
5	22 14·3	22 46·3	23 17·8	4	335·3	354·7	13·7
9	15 32·8	16 4·9	16 35·3	4	334·8	354·2	12·6
16	16 17·3	16 49·3	17 19·8	5	335·6	354·9	13·3
23	17 4·3	17 36·3	18 7·3	5	337·9	357·2	16·0
26	14 30·1	15 2·8	15 34·3	4	336·3	356·1	15·1
28	16 10·2	16 41·5	17 12·8	4	338·0	356·9	15·8
Oct. 1		14 9·8	14 41·4	5		356·8	16·0
3		15 52·5		3		0·1	
5	16 54·9	17 27·4	17 58·3	5	338·9	358·5	17·2
8	14 21·6	14 53·3	15 24·8	4	337·9	357·0	16·1
10	15 59·3	16 32·1	17 2·3	4	338·0	357·9	16·1
13	13 30·6	14 0·8		3	339·8	358·2	
17	16 41·8			3	337·5		
22		16 20·6	16 52·4	4		357·2	16·5
29	16 33·8	17 5·8	17 36·9	5	338·7	358·1	16·9
Nov. 6	13 12·8	13 43·7		3	341·0	359·6	
13	13 52·9	14 24·7	14 54·1	5	338·3	357·5	15·3
15	15 33·6	16 3·9		3	340·0	358·3	
18	13 0·8	13 33·0	14 3·8	3	338·9	358·3	16·9
20	14 36·3	15 8·9	15 39·3	3	337·4	357·1	15·5
22	16 19·0	16 50·1	17 20·3	4	340·2	359·0	17·3
Dec. 2	14 32·9	15 2·8	15 33·3	4	339·7	357·8	16·3
12	12 48·7	13 19·9	13 50·8	5	340·0	358·9	17·6
14	14 30·0	14 59·5	15 31·7	5	341·9	359·7	19·2
1881							
Jan. 5	12 38·4	13 9·3	13 40·6	5	340·3	358·9	17·8
10		12 20·5	12 50·8	3		0·6	18·9
12		14 0·8		3		1·6	
15	10 57·3	11 28·6	11 57·8	3	341·4	0·3	18·0
26	15 3·1	15 34·3		4	342·2	1·0	
27	10 53·0	11 24·3	11 55·3	5	341·2	0·2	18·9
Feb. 10	12 31·3	13 0·4	13 30·5	5	343·2	0·8	19·0
15	11 44·3	12 13·3	12 41·8	4	345·7	3·2	20·4
22		13 5·3	13 33·8	3		5·8	23·0

Addition to a Paper on the "Effect on the Moon's Movement in Latitude produced by the slow Change of Position of the Plane of the Ecliptic." By Sir G. B. Airy, K.C.B., Astronomer Royal.

In the paper in the *Monthly Notice* of March 1881 on the "Effect on the Moon's Movement in Latitude &c.," there is a trifling omission.

In page 265, line 3, the value to be used for R is not the radius vector of the centre of gravity of the luno-terrestrial system round the Sun, but R_m, the Moon's distance from the Sun. The former has inadvertently been used.

For correction, the expression of the force acting on the Moon must be multiplied by

$$\left(\frac{R}{Rm}\right)^3, \text{ or } 1 + \cdot00780 \cdot \cos \left| \overline{nt - Nt} \right|.$$

This introduces into the final equation the three terms

$$+ \cdot0083930 \cdot Bt \times \{ + \cdot00390 \cdot \sin \left| \overline{2nt - Nt - C} \right| - \cdot00390 \cdot \sin \left| \overline{2nt - 3Nt + C} \right| + \cdot00780 \cdot \sin \left| \overline{Nt - C} \right| \}$$

which are not much increased in integration.

In magnitude, these terms are unimportant. Theoretically, I can hardly conceive such terms to exist. It seems not improbable that they are neutralised, as to their effect normal to the instantaneous ecliptic, by the resolved part of forces parallel to the fixed ecliptic. I have not had leisure to examine into this point.

Royal Observatory, Greenwich:
1881, *April* 19.

Note on the Physical Libration of the Moon. By Dr. E. Hartwig.

Die am hiesigen Heliometer ausgeführte Beobachtungsreihe für Bestimmung der physischen Libration des Mondes ist in den *Monthly Notices of the Royal Astronomical Society* mehrfach erwähnt worden, und in dem neuesten Hefte findet sich über dieselbe eine Note von Professor Pritchard. Dieser Umstand veranlasst mich zu einigen nachträglichen Bemerkungen, welche ich der Royal Astronomical Society vorzulegen mir erlaube.

Bei der Ausgleichung der Bedingungsgleichungen nach der Methode der kleinsten Quadrate habe ich in der Berücksichtigung des Gewichtes der Breitengleichungen gegenüber der Längengleichungen einen Fehler begangen. Die Normalgleichungen in Breite habe ich nämlich statt mit dem Gewichte selbst mit dem Quadrat des Gewichtes multiplicirt, welcher Fehler weniger die

Werthe der zu bestimmenden Unbekannten, als vielmehr die Werthe ihrer wahrscheinlichen Fehler, d. h. die Schätzung der Genauigkeit beeinflusst.

Die richtige Ausgleichung mit dem aus der zweiten Ausgleichung sich ergebenden Gewichtsverhältnisse 2, 3 : 1 der Breitengleichungen zu den Längengleichungen liefert die folgenden Werthe der acht Unbekannten, welche von den früheren nicht um $\frac{1}{10}$, im Maximo bei dem Werthe von F $\frac{1}{4}$ des wahrscheinlichen Fehlers abweichen:

			w. F.
F	1° 36′ 39″		± 139″3
λ	−5 10 58		± 23·8
β	−3 10 23		± 12·5
$a_0 \sin A_0$	+	62·3	± 22·8
$a_0 \cos A_0$	−	0·9	± 35·1
$b_0 \sin B'$	+	294·6	± 131·7
$b_0 \cos B'$	+	56·4	± 47·6
F	+	954·8	+ 113·4
f		0·507	

Der wahrscheinliche Fehler einer Beobachtung ergibt sich zu ±88″·2, von der Erde gesehen ±0″·40.

Der gleiche Fehler ist auch in der früheren Königsberger Reihe begangen worden. Dort sind also die Breitengleichungen statt mit dem Gewichte $\sqrt{10}$ mit dem Gewichte 10 behandelt worden. Die richtige Ausgleichung mit dem Gewichtsverhältnisse 2, 7 : 1 ergibt die folgenden Werthe:

			w. F.
F	1° 32′ 39″		± 255″5
λ	−5 13 22		± 49·8
β	−3 11 3		± 17·2
$a_0 \sin A_0$	−	59·8	± 50·6
$a_0 \cos A_0$	+	173·2	± 63·0
$b_0 \sin B'$	−	12·0	± 229·0
$b_0 \sin B'$	+	16·9	± 116·9
F	+	671·8	± 192·2
f		0·357	

Der wahrscheinliche Fehler einer Beobachtung selenocentrisch folgt zu ±132″·2, geocentrisch + 0″·61.

Strassburg:
1881, *Mai* 4.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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E. J. STONE, Esq., Vice President, in the Chair.

Dr. Hugo Gyldén, The Observatory, Stockholm;
Prof. Edward C. Pickering, Harvard College, U.S.;
Signor W. Tempel, The Observatory, Arcetri, Florence;
Prof. F. Tietjen, The Observatory, Berlin; and
Mons. F. Tisserand, Paris;

were balloted for and duly elected Associates of the Society.

The Geographical Position of the Observatory of the John C. Green School of Science at Princeton, N. Jersey, U.S.A. By Professor C. A. Young.

This Observatory was established in 1877 mainly for the purpose of instruction in Practical Astronomy. It is equipped with a 9½-inch Equatoreal; a Meridian Circle of 4 inches aperture, with circles 2 feet in diameter; two Transit instruments of 3 and 2¾ inches aperture; and a 3-inch Prime Vertical Transit; also with the usual accessories of clocks, chronograph, micrometers, sextants, &c.

The longitude of the Meridian Circle was determined in 1878–79 by telegraphic connection with Washington. At that place the observations and reductions were made almost entirely by Asst. H. M. Paul; at Princeton the observations were made by myself, and the reductions were made for the most part by my assistant, Mr. M. McNeill, under my direction. The details of the work will appear in full in the "Washington Observations" for 1879.

Clock-comparisons were obtained on *seven* nights. After applying all corrections, that for my own personal equation being probably the most uncertain of them, these comparisons give the following results for the difference of Longitude between the *Dome of the Naval Observatory* at Washington and the Princeton *Meridian Circle*.

Date.		Δλ	Weight.
		m s	
1879, Jan.	18	−9 34·716	½
	22	34·491	1
	23	34·618	1
	30	34·556	1
March	5	34·401	½
	7	34·546	1
	12	34·438	1
Mean by weights	...	−9 34·538 ± ·021	

The discordances are larger than desirable, and are probably due mainly to my own unsteady personal equation, resulting from ill health at the time of the observations; partly also to a serious instability in the Y's of the Princeton Meridian Circle, which was not discovered and remedied until the work was nearly finished. It is believed, however, that the final result is substantially correct.

For the Latitude of the Observatory we have five closely-accordant determinations, by two different observers with three instruments. Four of the determinations were made by the Zenith-telescope method, and one by the Prime-vertical instrument.

Series I. consists of 82 observations of 28 pairs of stars on 9 nights, made by A. S. Flint in the early part of 1879, with the instrument known in the Observatory as the "Kahler Transit." It was made by Kahler of Washington, and has a "broken telescope" of $2\frac{3}{4}$ inches aperture and 31 inches focal length. It is provided with a reversing apparatus, with a delicate "latitude level" attached to the telescope tube, and with a declination micrometer; so that the instrument can be used as a zenith-telescope in the manner first proposed by Professor Lyman. It is in every way a most convenient and satisfactory instrument.

Series II. consists of 59 observations of 42 pairs, made by Mr. McNeill on two nights in May 1880.

Series III. consists of 114 observations of 49 pairs by the same observer and instrument on three nights in Sept. 1880.

Series IV. consists of 33 observations of 29 pairs by the same observer on two nights in Sept. 1880, made with the so-called Fauth Transit. This instrument, by Fauth and Co. of Washington, is a Transit of the ordinary form with a straight telescope of 3 inches aperture and 45 inches focus, carrying a diagonal

eyepiece and micrometer. It has a reversing apparatus, and a latitude level attached, not to the tube of the telescope, but to one of the 12-inch finding-circles on the axis. The field of view is rather restricted, and this renders the instrument much less convenient than the other for latitude work, though it is better for time determinations.

All of these observations were reduced under my direction by Mr. McNeill, and in substantially the same way. With approximate values of the micrometer constant and latitude, equations of condition were formed; the solutions of these equations by least squares furnished the necessary corrections to the assumed values. Thus the results depend entirely upon the star-declinations adopted and the observations themselves, without involving any subsidiary observations, made under different circumstances to determine the micrometer constant.

The declinations used were taken from the following Catalogues in the indicated order of preference.

1. Boss's Catalogue of 500 stars, published by the U.S. Northern Boundary Commission in 1878.

2. Auwers' "Fundamental Catalog" of 539 stars (Astron. Gesellschaft, 1879).

3. Safford's Catalogue of 2,018 stars, published by the U.S. War Department.

4. Catalogue of 2,179 stars contained in the Report of the U.S. Coast Survey for 1876.

The fifth series consists of 37 Prime-vertical latitude determinations from 11 different stars, made by Mr. McNeill in Jan. and Feb. 1881. The instrument is an old (but excellent) 3-inch Transit by Berge of London, fitted with an iron stand and reversing apparatus by Stackpole of New York, and with a striding level by Kahler.

26 of the 37 observations were made and reduced by Struve's method, the star being caught on both sides of the meridian; 15 of the 26 were observations of *α Lyræ*.

The pier of the Kahler instrument is in the same latitude as the Meridian Circle; the Fauth Transit is 0″·025 north, and the Prime-vertical instrument 0″·186 south. Applying the necessary corrections, the five results for the latitude of the *Meridian Circle* are as follow, viz.—

Series I.	$40^\circ\ 20'\ 57''\cdot791 \pm 0''\cdot148$
„ II.	$57''\cdot763 \pm 0''\cdot116$
„ III.	$57''\cdot815 \pm 0''\cdot067$
„ IV.	$57''\cdot771 \pm 0''\cdot173$
„ V.	$57''\cdot775 \pm 0''\cdot088$

The probable errors are deduced from the residuals of the equations of condition combined with the probable errors of the star declinations.

Giving each result a weight inversely proportional to the square of its probable error, and taking the weighted mean, we get as the astronomical Latitude of the Princeton Meridian Circle 40° 20′ 57″·792 ± 0″·044.

I am indebted to the Superintendent of the U.S. Coast and Geodetic Survey for data which determine our *Geodetic* Latitude and Longitude, when taken in connection with a survey made in 1877 under the direction of my colleague Professor McMillan, for the purpose of ascertaining our distance and direction from the cupola of the Theological Seminary, which is a Coast Survey point. "My Meridian Circle was found to be 2869ft·8 east, and 1130ft·9 north, of the seminary; giving a difference of longitude of 37″·04, and a difference of latitude of 11″·17."

According to the latest Coast Survey data, the geodetic Latitude and Longitude (from the meridian of Greenwich) of the Seminary cupola are respectively 40° 20′ 42″·842 and 74° 39′ 53″·942. For the astronomical Longitude of the Meridian Circle from Greenwich we get 74° 39′ 22″·47 by applying the observed difference of longitude (9m 34s·538) to the adjusted telegraphic longitude of the Washington Dome as adopted by the Coast Survey—viz. 77° 03′ 00″·54.

We thus have for the Princeton Meridian Circle the following data:—

	Latitude.	Longitude from Greenwich.
	° ′ ″	° ′ ″
Astronomical ...	40 20 57·79	74 39 22·47
Geodetic	40 20 54·01	74 39 16·90
A−G	+ 3 78	+ 5·57

Reducing these deviations to a single one, it appears that gravity at Princeton is deflected towards the south-east by 5″·68 according to the Coast Survey determinations which are based upon Clarke's spheroid.

This, I understand, is perfectly in accordance with results obtained by the Coast Survey at other points in this region.

A line of levels carried down to the Delaware and Raritan Canal shows the axis of the Circle to be 214·5 feet above the mean sea level in Raritan Bay.

In order to prevent confusion it may be well to add that the *Halsted Observatory* in which the great Equatoreal of 23 inches diameter is soon to be mounted, is an entirely different building from the School of Science Observatory. It is about 2,000 feet distant to the west and north, but its position has not yet been carefully determined.

Princeton, N.J.:
1881, *May* 13.

Note on some Points connected with the Determination of the Coefficient of the Parallactic Inequality. By E. J. Stone, Esq.

There are three points raised by Messrs. Campbell and Neison in their paper "On the Parallactic Inequality" (*Monthly Notices*, vol. xli. No. 5), to which I should wish briefly to reply.

1. The correction 1″·56, which I have applied to the coefficient of the parallactic inequality deduced directly from a comparison between the Greenwich observations and Burckhardt's longitudes of the Moon, is the mean correction which is supposed to be required for errors in Burckhardt's tables when observations extending over nine years, and thus including a revolution of the apse, are grouped together; and Burckhardt's tables were used by me for the determination of the coefficient of the parallactic inequality in groups of nine-year periods. If Messrs. Campbell and Neison could show that this correction was in error 0″·5, for such a group of observations, they would have shown that the resulting value of the solar parallax was in error between 0″·03 and 0″·04. But these gentlemen have shown nothing of the kind; and I feel certain that no error, to this extent, has been committed through the neglect of any recognised lunar inequalities. I believe the correction applied was more than sufficiently accurate for the objects which I had in view. I did not expect to fix the value of the coefficient of the parallactic inequality, with the materials available in 1867, within 0″·25, and, although the result which Messrs. Campbell and Neison have recently found agrees very closely with that which I found in 1867, I am still very doubtful whether we do, as a matter of fact, know the value of the coefficient within a quarter of a second of arc.

2. I am afraid that Messrs. Campbell and Neison have hardly appreciated the force of the objection which I raised, and I think substantiated, against the logical completeness of their investigation in the *Monthly Notices*, vol. xl. Nos. 7 and 8.

I have, at present, seen no proof that the differences, between the optical semi-diameters of the Moon near the Full and Quarters, in the winter and summer months respectively, bear to each other a ratio of 0·25 to 0·75, or 1 : 3, rather than a ratio of 1 : 4, or 3 : 8; or 1 : 2. But waiving this point: what then? The investigation of Messrs. Campbell and Neison led them to the values of I, the supposed constant of irradiation, which I have given in the *Monthly Notices*, Nov. 1880, vol. xli. No. 1. These values of I are very nearly, indeed, as discordant as the values deduced from an investigation which Messrs. Campbell and Neison have most strongly condemned, simply on account of the discordant values of I to which it led. It appears to me, therefore, that the authors might with equal justice, and on the same grounds, have condemned their own investigation; and I entirely fail to see how

they have vindicated their investigation from the demurrer raised against it.

There is no doubt whatever that when we possess a perfect Lunar Theory, with all the six constants accurately determined, and are in possession of perfect methods for the reduction of our observations, including the elimination of all systematic errors, instrumental and personal, we shall free our equations of conditions from all discordances; all I maintain is that Messrs. Campbell and Neison have not yet succeeded in the elimination of such discordances, and that, as a matter of fact, their attempts to do so in this matter of irradiation have not, according to their own standard, been successful.

3. I am afraid that I must take exception to the statement made by Messrs. Campbell and Neison that "the entire series of the Greenwich meridian observations of the Moon unite in supporting the hypothesis of a forty-five-year inequality." To support this statement, the following results, as coefficients of the parallactic inequality corrected for the forty-five-year inequality, are given:—

"From observations

1851–1858	124″·23
1862–1869	124·09
1870–1876	124·13.

"Airy's value

1751–1846	124″·13."

These results undoubtedly strongly support the statement made; but the evidence brought forward will not, I fear, bear examination. The division of the group of observations 1862–1876, adopted by the authors, can afford no independent evidence for or against the forty-five-year inequality. But if we divide the observations into two groups of seven years each, with corrections for the inequality above the mean correction for the whole group, 1″·02, and equal to or below that mean, we shall find for the first group a value of the coefficient 125″·07, and for the second a value of 125″·20. These two results only differ by 0″·13, but if we introduce the correction for the supposed forty-five-year inequality, the results will be found to differ by 0″·39.

So far, therefore, as the results given by Messrs. Campbell and Neison for the values of the parallactic inequality can be trusted to afford evidence in seven-year groups, upon questions affecting a quarter of a second of arc, these results show that the value of the parallactic inequality found from the observations about 1870 does not require to be diminished by 1″·02. And, with respect to the results given for the observations 1851–1858, the authors have applied a correction for semi-diameter deduced as a correction to Hansen's semi-diameter, and

have apparently overlooked the fact that the observations 1851–1858 were either reduced directly with Adams' value of the semi-diameter, or corrected by the Astronomer Royal for the differences between the adopted values and Adams' value of the semi-diameter. As Adams' value is quite half a second larger than Hansen's value, the coefficient found by the authors requires to be increased from this cause by half a second. Nor is this all. In the paper, vol. xl. 1880, May and June, Nos. 7 and 8, the authors have increased the value of the parallactic inequality for error of Hansen's semi-diameter for the Transit Circle observations by $0''\cdot29$ in the mean group, whilst in the case of the observations 1851–1858 they have decreased the coefficient by $0''\cdot18$. These changes are certainly not satisfactory. So far as the observations 1854–1858 are concerned, which were made with the chronographic method, the same mean correction should be applied. And from a direct investigation I find that the correction required for error of adopted semi-diameter is $0''\cdot79$; which agrees very closely with the result above indicated, viz. :—

Excess of Adams over Hansen ...	$+0''\cdot52$
Correction to Hansen	$0\cdot29$
	$0\cdot81$

It appears to me that accepting all the authors' results except the correction to semi-diameter—which is certainly not satisfactory —the coefficient of the parallactic inequality from the observations 1851–1858, instead of agreeing with the result 1862–1876 after correction for the forty-five-year inequality is greater than that result by one second of arc, and is made more discordant by the application of the hypothetical inequality.

The value given for the earlier observations 1751–1846 (viz. $124''\cdot13$) is also to be received with caution.

The observations 1750–1810 give a value about $121''\cdot73$, whilst those from 1811–1851 give a value of $124''\cdot37$. The authors bridge over the difficulty presented by the large difference between these results by assuming that all the earlier observations require a constant correction of $1''\cdot6$; and this although there were several changes of instruments, observers, and methods of observing. Nor is this all. To reconcile the result thus obtained by this constant correction and the forty-five-year inequality, they have to assume that the result for the group of observations 1843–1851 requires to be diminished by $1''\cdot78$ for supposed errors of diameter. I cannot recognise the legitimacy of these steps. That the earlier observations, as reduced, show the existence of some systematic discordance with a period of about forty-five years was pointed out many years ago by the Astronomer Royal; but that such a discordance is due to an inequality in the theoretical determination of the Moon's longitude $=1''\cdot20 \sin [D-8(y-1825\cdot5)]$, appears to me most doubtful,

and is, I believe, quite unproven. It would not account for the known facts connected with these earlier observations. It fails to afford any explanation of the large constant differences between these earlier results and those more recently obtained. It fails to represent the results of the observations about 1843–1851. And it does not appear to be supported by the observations 1851–1876, and it is certainly not supported by the results derived from the Altazimuth observations.

Note on the Effect of Atmospheric Dispersion on the Determination of the Solar Parallax by Meridian Observations of Mars. By E. J. Stone, Esq.

There is one point connected with the determination of the Solar Parallax from meridian observations of *Mars* which has been frequently asserted, but has not, so far as I am aware, been yet proved. It is asserted that the larger value of the solar parallax which has been found by this method is due to atmospheric dispersion. And this point has again been raised by Mr. Gill in the abstract of his paper on the solar parallax (vol. xli. No. 6):—

"The large values of π which have been determined from the meridian observations of *Mars* are explained by the fact that, at the altitudes at which the planet was observed in Europe in 1877, the limbs of *Mars* are spread by chromatic dispersion of the atmosphere into a fringe of coloured light 2″·2 broad. For the upper limb the violet part of the fringe is projected outside the disk of the planet, and for the lower limb the red part of the fringe is outside. In the field of a meridian instrument illuminated by white light, more of the violet than of the red false limb would be obliterated, and hence the observer will cut more deeply with the spider-web into the false violet than into the more glaring red limb. Thus all the zenith distances measured would be too great, and the resulting parallax would also be too great."

The general effects of chromatic dispersion should, like those of refraction, be much diminished, or generally destroyed, when the position of the planet is referred both to stars of lesser and greater zenith distances, since the light of these stars also suffers dispersion.

And, if this cause was sufficient to produce an effect of 0″·2 on the resulting value of the solar parallax, it might certainly be expected to be distinctly shown in the difference between the measured vertical and horizontal diameters of *Mars*. The vertical diameters would, it appears to me, be very sensibly the greater. I am not aware of any well-established discordance of the kind. And, as a matter of fact, of the twenty-six diameters of *Mars* measured simultaneously at Greenwich in 1877 the mean difference of the diameters is only 0″·25, and the horizontal diameter happens to be the larger.

Note on the Inequality in the Moon's Latitude which is due to the Secular Change of the Plane of the Ecliptic. By Prof. J. C. Adams, M.A.

The first theoretical explanation of this inequality was given by Hansen in the year 1849, in No. 685 of the *Astronomische Nachrichten*, just a year after the Astronomer Royal had pointed out, in a letter published in the same journal—*Beilage zu No.* 648—that such an inequality was clearly indicated by the observations. In the same paper Hansen shows that there is a small term in the Moon's longitude depending on the same cause, the coefficient of which amounts to about $0''{\cdot}5$, the inequality being proportional to the cosine of the longitude of the Moon's node. The existence of this inequality also had been indicated by the Astronomer Royal from the observations, though he assigns to it a somewhat larger coefficient.

The calculation of both these inequalities is given by Hansen somewhat more fully in p. 491, art. 176 of his *Darlegung*.

In 1853 I communicated to Mr. Godfray a simple theoretical explanation of the inequality in latitude, which he inserted in his "Elementary Treatise on the Lunar Theory." This explanation is there given in rather too compendious a form, and I propose in the course of this paper to present to the Society the same investigation, with some slight modification, together with some additional remarks, which will, I hope, render it clearer than before.

At the meeting of the Society in March last, the Astronomer Royal gave an investigation of the inequality in latitude based upon the equations supplied by the "Factorial Tables" of his "Numerical Lunar Theory." About one portion of this investigation I wish to make a remark which seems to be important.

The Astronomer Royal forms his equations with reference to the *fixed* ecliptic, and, by integrating them, derives the value of the disturbed latitude above the *fixed* ecliptic, whence the latitude above the variable ecliptic is immediately deduced.

The latitude so found contains not only the inequality in latitude required, but also the small residual terms

$$B\{\cdot 003 \sin \overline{|nt - C|} + \cdot 005 \sin \overline{|nt - 2Nt + C|}\},$$

which the Astronomer Royal rejects, attributing them to accidental errors in the last places of the decimals employed.

I shall presently attempt to show that these terms must indeed be rejected, though not for the reason here supposed, but because they are destroyed by other terms which would be found by a more complete investigation.

It should be remarked that if terms of the above form really existed, they would, notwithstanding the smallness of their numerical coefficients, ultimately become much more important than the other terms in which t does not occur in the coefficients.

I propose to prove that in the complete solution of the differential equations no terms of the above-mentioned form can occur, supposing the displacements of the plane of the ecliptic to be proportional to the first power of t. The method which I employ for this purpose is the following.

Instead of solving the differential equations of motion with reference to the *fixed ecliptic* and then transforming the results so as to make them apply to the *variable ecliptic*, I first transform the differential equations of motion, so as to make them refer to the *variable ecliptic*, and when this is done, it is found that the terms which contain t in their coefficients disappear completely from the differential equations, so that the solution may be effected by the ordinary methods without any difficulty.

Employing the same data and notation as the Astronomer Royal, and taking into account only the terms which are independent of the Moon's eccentricity and inclination, I find

$$\delta s = -1''\cdot424\cos(nt-C) + 0''\cdot048\cos(-nt+2Nt-C) - 0''\cdot007\cos(3nt-2Nt-C).$$

The reason why, in the result found by the Astronomer Royal, the terms which are multiplied by t do not completely destroy each other, as they ought to do, appears to be the following.

It is at once seen, from the form of the periodic terms to which the Astronomer Royal confines his attention, that his investigation is only complete with respect to the terms which are independent of the eccentricity and inclination of the Moon's orbit. In order to take the eccentricity and inclination into account, other periodic terms must be included, the arguments of which involve the Moon's mean anomaly and its mean distance from the node. From the combination of these terms with each other will arise terms with the same *arguments* as those which are independent of the eccentricity and inclination, while each of their *coefficients* contains the square of one of these elements as a factor. Hence it is clear that terms of this order are omitted in the investigation.

On the other hand, a slight examination shows that the coefficients in the Astronomer Royal's expressions for

$$\frac{r}{a}\cos l \text{ and } v,$$

as well as in the quantities taken from his Factorial Table, include very sensible portions depending on the squares of the eccentricity and inclination.

In fact, it is plain that this must necessarily be the case since the quantities in question are functions of the Moon's *actual* coordinates, in which the numerical values of those elements are essentially involved.

Now, if terms depending on the squares of the eccentricity and

inclination were either wholly neglected, or completely taken into account, the terms which are multiplied by t would be found identically to destroy each other; but if, as in the present case, such terms are taken into account in one part of the investigation, and omitted in another part, it will follow that some of the terms multiplied by t will remain outstanding.

A curious circumstance relating to this inequality of latitude remains to be noticed.

In the *Mécanique Céleste*, tome 3, p. 185, Laplace proves that the plane of the Earth's orbit in its secular motion carries the plane of the Moon's orbit with it, so that the inclination of the Moon's orbit to the variable ecliptic is not liable to any secular variation.

In the same place he finds an analytical expression for the perturbation of latitude in reference to the variable ecliptic which is caused by the secular change in that plane.

Now the point to be noticed is that this analytical expression given by Laplace requires only the very slightest possible development to furnish for the inequality in question a result which is identical with the value given by the formula of Hansen, in which displacements of the ecliptic varying not only as the first but also as the second power of the time are taken into account. It is true that Laplace imagined that this inequality would turn out to be insensible, but this was only because he had not attempted to turn his formula into numbers.

Analysis.

I. Investigation of the inequality in the Moon's latitude which is due to the secular motion of the plane of the ecliptic, making the same suppositions and employing the same data as the Astronomer Royal.

At the time t let x, y, z be the rectangular coordinates of the Moon, and x', y' those of the Sun, referred to the Earth's centre as origin, the variable plane of the ecliptic at the same time being taken as the plane of $x\ y$.

Also at the time t let ξ, η, ζ be the rectangular coordinates of the Moon, and ξ', η', ζ' those of the Sun, taking the fixed plane of the ecliptic corresponding to $t=0$ as the plane of $\xi\ \eta$.

For greater simplicity we will suppose, with the Astronomer Royal, that the variable ecliptic intersects the fixed ecliptic in a fixed line, and that the angle between these two planes is proportional to the time.

Let this fixed line be taken as the axis of x and also as the axis of ξ, and let ωt be the angle between the variable and the fixed ecliptic, then the relations between the coordinates belonging to the two systems will be

$$\xi = x,$$

$$\eta = y \cos \omega t - z \sin \omega t,$$

$$\zeta = z \cos \omega t + y \sin \omega t,$$

and similarly

$$\xi' = x,$$
$$\eta' = y' \cos \omega t,$$
$$\zeta' = y' \sin \omega t.$$

Let r be the Moon's radius vector at time t, r' that of the Sun, m' the Sun's mass, μ the sum of the masses of the Earth and Moon, and R the disturbing function, then we have

$$R = -\frac{1}{2}\frac{m'r^2}{r'^3} + \frac{3}{2}\frac{m'(\xi\xi' + \eta\eta' + \zeta\zeta')^2}{r'^5},$$

and the equations of motion, with reference to the fixed ecliptic, will be

$$\frac{d^2\xi}{dt^2} + \frac{\mu\xi}{r^3} = \frac{dR}{d\xi},$$
$$\frac{d^2\eta}{dt^2} + \frac{\mu\eta}{r^3} = \frac{dR}{d\eta},$$
$$\frac{d^2\zeta}{dt^2} + \frac{\mu\zeta}{r^3} = \frac{dR}{d\zeta},$$

or, substituting the values of

$$\frac{dR}{d\xi}, \quad \frac{dR}{d\eta} \text{ and } \frac{dR}{d\zeta},$$

(1) $$\frac{d^2\xi}{dt^2} + \frac{\mu\xi}{r^3} = -\frac{m'\xi}{r'^3} + \frac{3m'\xi'}{r'^5}(\xi\xi' + \eta\eta' + \zeta\zeta'),$$

(2) $$\frac{d^2\eta}{dt^2} + \frac{\mu\eta}{r^3} = -\frac{m'\eta}{r'^3} + \frac{3m'\eta'}{r'^5}(\xi\xi' + \eta\eta' + \zeta\zeta'),$$

(3) $$\frac{d^2\zeta}{dt^2} + \frac{\mu\zeta}{r^3} = -\frac{m'\zeta}{r'^3} + \frac{3m'\zeta'}{r'^5}(\xi\xi' + \eta\eta' + \zeta\zeta').$$

Now we have, from the values of η and ζ above given,

$$\frac{d^2\eta}{dt^2} = \left(\frac{d^2y}{dt^2} - 2\omega\frac{dz}{dt} - \omega^2 y\right)\cos \omega t - \left(\frac{d^2z}{dt^2} + 2\omega\frac{dy}{dt} - \omega^2 z\right)\sin \omega t,$$

and

$$\frac{d^2\zeta}{dt^2} = \left(\frac{d^2z}{dt^2} + 2\omega\frac{dy}{dt} - \omega^2 z\right)\cos \omega t + \left(\frac{d^2y}{dt^2} - 2\omega\frac{dz}{dt} - \omega^2 y\right)\sin \omega t,$$

and therefore

$$\frac{d^2\eta}{dt^2}\cos \omega t + \frac{d^2\zeta}{dt^2}\sin \omega t = \frac{d^2y}{dt^2} - 2\omega\frac{dz}{dt} - \omega^2 y,$$
$$\frac{d^2\zeta}{dt^2}\cos \omega t - \frac{d^2\eta}{dt^2}\sin \omega t = \frac{d^2z}{dt^2} + 2\omega\frac{dy}{dt} - \omega^2 z.$$

Now substitute for ξ, η, ζ and ξ', η', ζ' their values in terms of x, y, z and x', y' respectively, in

(1),

(2) $\cos \omega t + (3) \sin \omega t$,

(3) $\cos \omega t - (2) \sin \omega t$,

bearing in mind that

$$\xi\xi' + \eta\eta' + \zeta\zeta' = xx' + yy',$$

since each of these quantities represents

$$rr' \cos (r, r'),$$

and we have

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} = -\frac{m'x}{r'^3} + \frac{3m'x'}{r'^5}(xx' + yy'),$$

$$\frac{d^2y}{dt^2} - 2\omega\frac{dx}{dt} - \omega^2 y + \frac{\mu y}{r^3} = -\frac{m'y}{r'^3} + \frac{3m'y'}{r'^5}(xx' + yy'),$$

$$\frac{d^2z}{dt^2} + 2\omega\frac{dy}{dt} - \omega^2 z + \frac{\mu z}{r^3} = -\frac{m'z}{r'^3},$$

which are the equations of the Moon's motion, with reference to the variable ecliptic.

The motion of the ecliptic is so slow (that is, ω is so small) that the terms involving ω^2 may be neglected.

We will now change the notation by writing for the Moon's coordinates $x+\delta x$, $y+\delta y$, and $z+\delta z$, instead of x, y, z respectively, in which expressions the new quantities x, y, z are taken so as to satisfy the equations of motion

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} = -\frac{m'x}{r'^3} + \frac{3m'x'}{r'^5}(xx' + yy'),$$

$$\frac{d^2y}{dt^2} + \frac{\mu y}{r^3} = -\frac{m'y}{r'^3} + \frac{3m'y'}{r'^5}(xx' + yy'),$$

$$\frac{d^2z}{dt^2} + \frac{\mu z}{r^3} = -\frac{m'z}{r'^3},$$

which are those of the ordinary lunar theory, in which the motion of the ecliptic is not taken into account, so that x, y, z may be supposed to be known functions of t.

Hence the equations for determining the small increments δx, δy, δz of the coordinates, which are due to the motion of the ecliptic, are the following:

$$\frac{d^2\delta x}{dt^2} + \left(\frac{\mu}{r^3} + \frac{m'}{r'^3}\right)\delta x - \frac{3\mu x}{r^5}\left(x\delta x + y\delta y + z\delta z\right) = \frac{3m'x'}{r'^5}\left(x'\delta x + y'\delta y\right),$$

$$\frac{d^2\delta y}{dt^2} + \left(\frac{\mu}{r^3} + \frac{m'}{r'^3}\right)\delta y - \frac{3\mu y}{r^5}\left(x\delta x + y\delta y + z\delta z\right) - 2\omega\frac{dz}{dt} = \frac{3m'y'}{r'^5}\left(x'\delta x + y'\delta y\right),$$

$$\frac{d^2dz}{dt^2} + \left(\frac{\mu}{r^3} + \frac{m'}{r'^3}\right)\delta z - \frac{3\mu z}{r^5}\left(x\delta x + y\delta y + z\delta z\right) + 2\omega\frac{dy}{dt} = 0.$$

We may remark that no terms involving arbitrary constants need be added to the values of δx, δy, δz, since these may be supposed to be already included in the values of x, y, z.

Hence we may choose for δx, δy, δz any particular values which satisfy these differential equations, and we may consider these values to contain ω as a factor throughout.

If γ denote the sine of the mean inclination of the Moon's orbit, the value of z, and therefore that of $\frac{dz}{dt}$, will contain γ as a factor throughout. Hence the form of the first two of these differential equations shows that the values of δx, δy, found under the above conditions, will contain $\gamma\omega$ as a factor throughout, and therefore that the term

$$\frac{3\mu z}{r^5}\left(x\delta x + y\delta y + z\delta z\right),$$

which occurs in the third differential equation, will contain the factor $\gamma^2\omega$ throughout.

If, therefore, we neglect the square of γ, the equation for δz takes the simple form

$$\frac{d^2\delta z}{dt^2} + \left(\frac{\mu}{r^3} + \frac{m'}{r'^3}\right)\delta z + 2\omega\frac{dy}{dt} = 0.$$

Now let θ be the Moon's longitude at time t measured from the axis of x, that is from the line of intersection of the variable and of the fixed ecliptic.

Also let nt and $n't$ be the mean longitudes of the Moon and the Sun, omitting, for the sake of brevity in writing, the constants which always accompany nt and $n't$ respectively.

For the sake of simplicity, we will now neglect the eccentricities of the two orbits as well as their mutual inclination.

In this case we have, with abundant accuracy for our present purpose,

$$r = 0{\cdot}99911{,}92 - 0{\cdot}00717{,}34\cos 2(nt-n't) - 0{\cdot}00002{,}00\cos 4(nt-n't),$$
$$\theta = nt \qquad + 0{\cdot}01021{,}14\sin 2(nt-n't) + 0{\cdot}00004{,}24\sin 4(nt-n't),$$

where, as in my paper in the *Monthly Notices*, vol. xxxviii. p. 46, the angles are expressed in the circular measure, and the unit of distance is the mean distance in an undisturbed orbit which would be described by the Moon about the Earth in its actual periodic time.

Hence we have, as in the paper referred to—

$$\mu = n^2, \text{ and } \frac{m'}{r'^3} = n'^2.$$

Now choose the unit of time such that

$$n - n' = 1;$$

therefore, since in the case of the Moon

$$\frac{n'}{n} = 0{\cdot}07480{,}13,$$

we have

$$n' = 0{\cdot}08084{,}89,$$

and

$$n = 1{\cdot}08084{,}89.$$

From the values of r and θ above given, it is readily found that

$$\begin{aligned} y = r\sin\theta = {} & -0{\cdot}00868{,}79\sin(-nt+2n't) \\ & +0{\cdot}99909{,}31\sin nt \\ & +0{\cdot}00151{,}43\sin(3nt-2n't) \\ & +0{\cdot}00000{,}59\sin(5nt-4n't), \end{aligned}$$

and hence that

$$\begin{aligned} \frac{dy}{dt} = {} & +0{\cdot}00798{.}55\cos(-nt+2n't) \\ & +1{\cdot}07986{,}87\cos nt \\ & +0{\cdot}00466{,}54\cos(3nt-2n't) \\ & +0{\cdot}00002{,}98\cos(5nt-4n't), \end{aligned}$$

and also, as in the paper referred to above,

$$\frac{\mu}{r^3}+\frac{m'}{r'^3} = 1{\cdot}17804{,}45+0{\cdot}02523{,}37\cos 2(nt-n't)+0{\cdot}00025{,}16\cos 4(nt-n't).$$

Hence the equation to be solved becomes

$$\begin{aligned} &\frac{d^2\delta z}{dt^2}+\delta z\left[1{\cdot}17804{,}45+0{\cdot}02523{.}37\cos 2(nt-n't)+0{\cdot}00025{,}16\cos 4(nt-n't)\right] \\ &\quad+\omega\left[0{\cdot}01597{,}1\cos(-nt+2n't)+2{\cdot}15973{,}7\cos nt\right. \\ &\qquad\left.+0{\cdot}00933{,}1\cos(3nt-2n't)+0{\cdot}00006{,}0\times\cos(5nt-4n't)\right]=0. \end{aligned}$$

Assume

$$\begin{aligned} \delta z = \omega[& c_{-2}\cos(-3nt+4n't)+c_{-1}\cos(-nt+2n't)+c_1\cos nt \\ & +c_2\cos(3nt-2n't)+c_3\cos(5nt-4n't)], \end{aligned}$$

then, by substituting for δz and equating coefficients of similar terms, we have

$$\begin{aligned} -7{\cdot}34339{,}8c_{-2}+0{\cdot}01261{,}7c_{-1}+0{\cdot}00012{,}6c_1 &= 0, \\ 0{\cdot}01261{,}7c_{-2}+0{\cdot}33320{,}6c_{-1}+0{\cdot}01261{,}7c_1+0{\cdot}00012{,}6c_2+0{\cdot}01597{,}1\omega &= 0, \\ 0{\cdot}00012{,}6c_{-2}+0{\cdot}01261{,}7c_{-1}+0{\cdot}00981{,}0c_1+0{\cdot}01261{,}7c_2+0{\cdot}00012{,}6c_3 & \\ +2{\cdot}15973{,}7\omega &= 0, \\ +0{\cdot}00012{,}6c_{-1}+0{\cdot}01261{,}7c_1-8{\cdot}31358{,}5c_2+0{\cdot}01261{,}7c_3+0{\cdot}00933{,}1\omega &= 0, \\ +0{\cdot}00012{,}6c_1+0{\cdot}01261{,}7c_2-24{\cdot}63698{,}1c_3+0{\cdot}00006{,}0\omega &= 0. \end{aligned}$$

If we find the values of c_{-3}, c_3, and c_5 from these equations in terms of the two remaining coefficients c_{-1} and c_1, which can be advantageously done, since c_{-3} has a large coefficient in the first equation, c_3 in the fourth and c_5 in the fifth equation, we find

$$c_{-3} = 0{\cdot}00171{,}8c_{-1} + 0{\cdot}00001{,}72\ c_1,$$
$$c_3 = 0{\cdot}00001{,}5c_{-1} + 0{\cdot}00151{,}76\ c_1 + 0{\cdot}00112{,}2\omega,$$
$$c_5 = + 0{\cdot}00000{,}589c_1 + 0{\cdot}00000{,}3\omega,$$

and substituting these values in the 2nd and 3rd equations, they become

$$0{\cdot}33322{,}76c_{-1} + 0{\cdot}01261{,}74c_1 + 0{\cdot}01597{,}1\omega = 0,$$
$$0{\cdot}01261{,}74c_{-1} + 0{\cdot}00982{,}925c_1 + 2{\cdot}15975{,}1\omega = 0.$$

Whence again, we find

$$c_{-1} = 8{\cdot}69441\omega,$$
$$c_1 = -230{\cdot}8866\ \omega,$$

from which by substitution we obtain

$$c_{-3} = 0{\cdot}01097\omega,$$
$$c_3 = -\ 0{\cdot}34915\omega,$$
$$c_5 = -\ 0{\cdot}00136\omega.$$

Hence the solution of the differential equation for δz is

$$\delta z = \omega\{0{\cdot}01097\cos(-3nt+4n't) + 8{\cdot}69441\cos(-nt+2n't) - 230{\cdot}8866\cos nt - 0{\cdot}34915\cos(3nt-2n't) - 0{\cdot}00136\cos(5nt-4n't)\}.$$

Here ω is expressed in terms of the circular measure, and δz in terms of the unit of length defined before.

If s denote the sine of the Moon's latitude,

$$s = \frac{z}{r},$$

and if δs be the change in s due to the secular change in the plane of the ecliptic, we have

$$\delta s = \frac{\delta x}{r},$$

since

$$\delta r = 0,$$

according to the suppositions made above.

Also

$$\frac{1}{r} = 1{\cdot}00090{,}74 + 0{\cdot}00718{,}65\cos 2(nt-n't) + 0{\cdot}00004{,}58\cos 4(nt-n't).$$

Hence by substitution

$$\delta s = \omega\{0{\cdot}0369\cos(-3nt+4n't)+7{\cdot}8727\cos(-nt+2n't)-231{\cdot}0661\cos nt \\ -1{\cdot}1789\cos(3nt-2n't)-0\,0079\cos(5nt-4n't)\}.$$

Also s being supposed very small, δs is equal to the circular measure of the change of the Moon's latitude due to the secular change in the plane of the ecliptic, and if we divide δs by sin 1″ we shall find the change of the latitude in *seconds*

$$=\frac{\omega}{\sin 1''}\{0{\cdot}0369\cos(-3nt+4n't)+7{\cdot}8727\cos(-nt+2n't)-231{\cdot}0661\cos nt \\ -1{\cdot}1789\cos(3nt-2n't)-0{\cdot}0079\cos(5nt-4n't)\}.$$

Now, according to the data adopted by the Astronomer Royal, the circular measure of the angular motion of the plane of the ecliptic in 1 year is 0·479 sin 1″.

Also 1 year is represented in our notation by the time

$$\frac{2\pi}{n'}.$$

Hence

$$\frac{2\pi}{n'}\omega = 0{\cdot}479\sin 1'',$$

and

$$\frac{\omega}{\sin 1''} = 0{\cdot}479\frac{n'}{2\pi} = 0{\cdot}00516.354.$$

Therefore the inequality of latitude expressed in seconds is

$$0''{\cdot}0002\cos(-3nt+4n't)+0''{\cdot}0485\cos(-nt+2n't)-1''{\cdot}4242\cos nt \\ -0''{\cdot}0073\cos(3nt-2n't).$$

In this expression the mean longitudes nt and $n't$ are reckoned from the node of the variable ecliptic upon the fixed ecliptic. If the mean longitudes are reckoned from the equinox in the ordinary way, and if C be the longitude of the above-mentioned node, we must replace nt and $n't$ in the above by $nt-C$ and $n't-C$ respectively, and the expression for the inequality in latitude becomes

$$0''{\cdot}0002\cos(-3nt+4n't-C)+0''{\cdot}0485\cos(-nt+2n't-C) \\ -1''{\cdot}4242\cos(nt-C)-0''{\cdot}0073\cos(3nt-2n't-C).$$

In the above investigation the quantities ω and C are supposed to be constant. If these be subject to small secular variations, the differential equations become a little less simple, but are easily formed, and the above solution will require the following modifications, viz.—

(1) Instead of the constant value of ω we must employ the variable value which is of the form

$$\omega_0 + \omega' t;$$

(2) The coefficients of the above expression will be very slightly changed by quantities which are proportional to

$$\omega \frac{dC}{dt};$$

(3) The expression for the inequality of latitude will contain extremely small additional terms of the form

$$\frac{d\omega}{dt}\{g_{-3}\sin(-3nt+4n't-C)+g_{-1}\sin(-nt+2n't-C)+g_1\sin(nt-C)$$
$$+g_3\sin(3nt-2n't-C)\};$$

that is to say, these terms will involve the *sines* instead of the *cosines* of the same arguments as before, and the coefficients of these new terms are proportional to

$$\frac{d\omega}{dt}.$$

II. Theoretical explanation of the same inequality, which was originally given, in substance, in Godfray's "Elementary Treatise on the Lunar Theory."

The general principle of this explanation may be very simply stated.

If, for a moment, we suppose the plane of the Moon's orbit to remain fixed, and imagine the plane of the ecliptic to turn through a very small given angle about a line in its own plane, this will give rise to corresponding small changes in the longitude of the Moon's node and in the inclination of the orbit to the ecliptic, and the magnitude of these changes will depend on the angular distance of the Moon's node from the line about which the ecliptic is supposed to be turning.

If now the planes of both orbits be supposed to vary continuously, the total changes in the longitude of the node and inclination of the orbit produced in an indefinitely small time will be found by adding together the changes respectively due to the motion of the plane of the ecliptic, and to the motion of the plane of the Moon's orbit with respect to the ecliptic when the latter is supposed to remain fixed during that small time. The motion last mentioned is given by the formulæ of the ordinary Lunar Theory, in terms of the disturbing force of the Sun. In consequence of the action of this force, the Moon's node gradually makes complete revolutions with respect to the line about which the ecliptic is turning, and the summation of all the momentary changes of node and inclination due to the motion of the ecliptic will produce periodic changes in those elements, the magnitudes of which, at any given time, like the momentary changes themselves, will depend on the angular distance, at that time, between the Moon's node and the line about which the ecliptic is turning.

The combined effect of these periodic changes in the position

of the node and in the inclination is to produce the inequality in latitude which is now under consideration.

The motion of the Moon's node is not uniform, but the principal inequalities by which that motion is affected have periods which are short compared with the time of revolution of the node.

Hence the periodic changes of node and inclination above described, will be accompanied by others which are due to the same cause, but which in consequence of the shortness of their periods will be comparatively unimportant, and the combined effect of these changes in the elements will be to add other terms which are equally unimportant to the expression of the inequality in latitude.

We proceed to find the analytical expressions for the changes in the longitude of the Moon's node and in the inclination of the orbit, due to the motion of the plane of the ecliptic, supposing the Moon's orbit itself to remain fixed.

Take C the longitude of the instantaneous axis about which the ecliptic is rotating at the time t,
ω the angular velocity of the ecliptic,
N the longitude of the Moon's node,
and i the inclination of the orbit, at the same instant.

Then, in the indefinitely small time δt, a point of the ecliptic situated in any arbitrary longitude L will move through an angular space

$$\omega \delta t \sin (L - C)$$

in a direction perpendicular to the ecliptic.

Hence the point of the ecliptic originally coincident with the node N will move through the space

$$\omega \delta t \sin (N - C)$$

perpendicular to the ecliptic.

And if δN be the consequent increase of the longitude of the node we have evidently from the figure,

$$\delta N = \omega \delta t \sin (N - C) \cot i$$

or

$$\frac{dN}{dt} = \omega \sin (N - C) \cot i.$$

Again, the point of the ecliptic 90° in advance of N will move through the space

$$\omega\delta t \sin (90° + N - C),$$

or

$$\omega\delta t \cos (N - C),$$

perpendicular to the ecliptic, and this quantity will measure the diminution in the inclination of the Moon's orbit.

Hence we have

$$\delta i = -\omega\delta t \cos (N - C),$$

or

$$\frac{di}{dt} = -\omega \cos (N - C).$$

Thus we have found the rates of change of the longitude of the Moon's node and of the inclination which are due to the motion of the ecliptic.

Now, suppose the formulæ which give the rates of change of the same two elements, with respect to a fixed ecliptic, which are due to the Sun's disturbing force, to be represented by

$$\frac{dN}{dt} = -c \cos i + F(\theta,\theta'),$$

and

$$\frac{di}{dt} = f(\theta,\theta'),$$

where $-c \cos i$ denotes the non-periodic term in $\frac{dN}{dt}$, c being approximately equal to $\frac{3}{4}\frac{n'^2}{n}$, and $F(\theta,\theta')$, $f(\theta,\theta')$ consist wholly of periodic terms which involve the longitudes θ, θ' of the Moon and Sun respectively, as well as the elements N and i.

Hence by what has been before said, if N', i' denote the longitude of the node and the inclination at the time t, with respect to the variable ecliptic, $\frac{dN'}{dt}$ and $\frac{di'}{dt}$ will be given by the following formulæ:—

$$\frac{dN'}{dt} = -c \cos i' + F(\theta,\theta') + \omega \sin (N' - C) \cot i',$$

$$\frac{di'}{dt} = f(\theta,\theta') - \omega \cos (N' - C),$$

in which $F(\theta,\theta')$, $f(\theta,\theta')$ now involve the elements N' and i', instead of N and i.

Now let N be the longitude of the node, and i the inclination at the time t, on the supposition that the ecliptic remains fixed,

all the other circumstances of the Moon's motion remaining unaltered; then we have as before

$$\frac{dN}{dt} = -c\cos i + F(\theta,\theta'),$$

$$\frac{di}{dt} = f(\theta,\theta').$$

Let

$$N' = N + \delta N.$$

and

$$i' = i + \delta i,$$

where δN and δi are entirely due to the motion of the ecliptic and therefore vanish with ω.*

Then neglecting the square of ω and supposing the value of θ, or the Moon's longitude, to remain unchanged, we have

$$\frac{d\,\delta N}{dt} = c\sin i\,\delta i + \left(\frac{dF}{dN}\right)\delta N + \left(\frac{dF}{di}\right)\delta i + \omega\sin(N-C)\cot i,$$

$$\frac{d\,\delta i}{dt} = \left(\frac{df}{dN}\right)\delta N + \left(\frac{df}{di}\right)\delta i - \omega\cos(N-C).$$

Now

$$\left(\frac{dF}{dN}\right),\ \left(\frac{dF}{di}\right),$$

and

$$\left(\frac{df}{dN}\right),\ \left(\frac{df}{di}\right),$$

are composed of periodic terms which have short periods compared with the time of revolution of the Moon's node—that is, with the period of the terms

$$\sin(N-C) \quad \text{and} \quad \cos(N-C).$$

Hence in integrating we may at first neglect the terms

$$\left(\frac{dF}{dN}\right)\delta N,\ \left(\frac{dF}{di}\right)\delta i \quad \text{and} \quad \left(\frac{df}{dN}\right)\delta N,\ \left(\frac{df}{di}\right)\delta i,$$

leaving them to be taken into account, if necessary, in a subsequent approximation.

For the same reason we may suppose cot i to be constant in integrating, and we make take

$$\frac{dN}{dt} = -c\cos i,$$

---

* It is hardly necessary to mention that δN and δi are here employed in a wholly different sense from that in which the same symbols were used, for a temporary purpose, in the earlier part of this investigation.

omitting the periodic term $\cdot F(\theta,\theta')$; and we may also suppose that ω and C are constants.

With these simplifications, we have

$$\frac{d\,\delta N}{dt} = c \sin i\, \delta i + \omega \cot i \sin (N-C),$$

$$\frac{d\,\delta i}{dt} = \frac{\omega}{c \cos i} \cos (N-C) \frac{dN}{dt}.$$

From the latter of these equations

$$\delta i = \frac{\omega}{c \cos i} \sin (N-C),$$

and substituting this value of δi in the former, we find

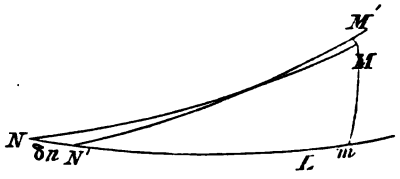
$$\frac{d\,\delta N}{dt} = \omega \tan i \sin (N-C) + \omega \cot i \sin (N-C),$$

$$= \frac{\omega}{\sin i \cos i} \sin (N-C),$$

$$= -\frac{\omega}{c \sin i \cos^2 i} \sin (N-C) \frac{dN}{dt},$$

and therefore

$$\delta N = \frac{\omega}{c \sin i \cos^2 i} \cos (N-C).$$



Now let NM be the Moon's orbit and M the place of the Moon as found from formulæ in which the plane of the ecliptic is supposed to be fixed, and let N′M′ be the Moon's orbit and M′ the place of the Moon at the same time taking into account the motion of the ecliptic.

Let $NM = \psi$, and $N'M' = \psi + \delta\psi$.

Also let s denote the sine of the Moon's latitude, and β the latitude itself, in the case when the ecliptic is supposed fixed;

And let $s + \delta s$ denote the sine of the latitude, and $\beta + \delta\beta$ the latitude itself, when the ecliptic is supposed to be variable.

Then

$$s = \sin i \sin \psi,$$

and

$$= \cos i \sin \psi\, \delta i + \sin i \cos \psi\, \delta\psi.$$

Now let us assume that MM′ is perpendicular to NM, in which case we shall have

$$\delta\psi = -\cos i\, \delta N,$$

and therefore

$$\delta s = \cos i \sin\psi\, \delta i - \sin i \cos i \cos\psi\, \delta N,$$

or substituting the values above found for δi and δN,

$$\delta s = \frac{\omega}{c} \sin\psi \sin(N-C) - \frac{\omega}{c\cos i} \cos\psi \cos(N-C).$$

But if θ denote the Moon's longitude, we have

$$\cos i \sin\psi = \cos\beta \sin(\theta - N),$$

and

$$\cos\psi = \cos\beta \cos(\theta - N).$$

Hence

$$\delta s = \frac{\omega}{c\cos i} \cos\beta\, [\sin(\theta-N)\sin(N-C) - \cos(\theta-N)\cos(N-C)],$$

or

$$\cos\beta\, \delta\beta = -\frac{\omega}{c\cos i} \cos\beta \cos(\theta - C),$$

and therefore

$$\delta\beta = -\frac{\omega}{c\cos i} \cos(\theta - C),$$

which is the inequality in latitude due to the motion of the ecliptic, expressed in the circular measure.

This value of $\delta\beta$ agrees exactly with that found in my article inserted in Godfray's Lunar Theory, since $c \cos i$ in this formula has the same signification as $\frac{I}{c}$ in Godfray, viz. the mean angular velocity of the Moon's node.

The steps, however, by which this result is arrived at, are slightly different in the two investigations. In the earlier one, the variation of $\cos i$ was neglected, and $\delta\psi$ was taken $= -\frac{\delta N}{\cos i}$, whereas in the present investigation the variation of $\cos i$ is taken into account, and $\delta\psi$ is taken $= -\cos i\, \delta N$, on the assumption that MM′ is perpendicular to NM.

It should be remarked that in both forms of this investigation, the neglect to take account of any variation of the Moon's radius vector and orbital longitude, due to the motion of the

ecliptic, may produce errors in the coefficient of the inequality in latitude which are of the order of the small quantity

$$\frac{\omega}{c} \sin^2 i,$$

so that the investigation is incompetent to decide such a question, for instance, as whether $\frac{\omega}{c \cos i}$ or $\frac{\omega}{c}$ is the more correct value of this coefficient.

The coefficient above found, expressed in seconds, is

$$\frac{\omega}{c \cos i \sin 1''}.$$

In order to evaluate this quantity numerically, we observe that $\frac{\omega}{c \cos i}$ is the ratio of two angular velocities: viz. the velocity of rotation of the plane of the ecliptic, and the mean angular velocity of the Moon's node; and in comparing these it is indifferent what unit of time is employed. According to the data adopted before, taking 1 year as the unit of time

$$\omega = 0{\cdot}479 \sin 1'', \text{ or } \frac{\omega}{\sin 1''} = 0{\cdot}479.$$

Also since the Moon's node takes about 18·6 years to perform a complete revolution

$$c \cos i = \frac{2\pi}{18{\cdot}6} \text{ nearly.}$$

Hence

$$\frac{\omega}{c \cos i \sin 1''} = \frac{0{\cdot}479 \times 18{\cdot}6}{2\pi}, \text{ expressed in seconds,}$$

$$= 1''{\cdot}42,$$

which agrees with the value of the coefficient of the principal term found in the former investigation.

The form above found for $\delta\beta$ suggests a very simple geometrical interpretation of this inequality in latitude.

If we suppose a fictitious ecliptic to be inclined to the true ecliptic at the angle $1''{\cdot}42$, the circular measure of which is $\frac{\omega}{c \cos i}$, and if we also suppose that the longitude of its ascending node on the true ecliptic is

$$90° + C,$$

then the elevation of the fictitious above the true ecliptic corresponding to the longitude θ will be

$$= \frac{\omega}{c \cos i} \sin (\theta - 90^\circ + \mathrm{C}),$$

$$= -\frac{\omega}{c \cos i} \cos (\theta - \mathrm{C}),$$

$$= \delta\beta.$$

Hence the latitude above the fictitious ecliptic will be equal to β, that is, the expression for the Moon's latitude with respect to the fictitious ecliptic is the same as the expression found for the latitude in the case when the ecliptic is taken to be a fixed plane.

This geometrical interpretation of the inequality was first given by Hansen.

III. Note on the *Mécanique Céleste*, tome iii. p. 185 (edition of 1802).

At any arbitrary point whose longitude is λ, Laplace takes the elevation of the variable ecliptic above the fixed plane of reference to be represented by

$$\Sigma k \sin (\lambda + it + \epsilon),$$

and he shows that if s_1 denotes the perturbation of the Moon's latitude with respect to the variable ecliptic which is due to the motion of that plane,

Then

$$s_1 = \Sigma \frac{(2i + i^2)k \sin (\nu + i\nu + \epsilon)}{\frac{3}{2}m^2 - 2i - i^2},$$

where ν denotes the Moon's longitude;
or

$$s_1 = \Sigma \left[\frac{2ki}{\frac{3}{2}m^2} + \frac{4ki^2}{(\frac{3}{2}m^2)^2} \right] \sin (\nu + i\nu + \epsilon)$$

very nearly, neglecting i^2 compared with i except when it is divided by an additional power of $\frac{3}{2}m^2$.

Or, replacing $i\nu$ by it

$$s_1 = \sin \nu \, \Sigma \left[\frac{2ki}{\frac{3}{2}m^2} + \frac{4ki^2}{(\frac{3}{2}m^2)^2} \right] \cos (it + \epsilon)$$

$$+ \cos \nu \, \Sigma \left[\frac{2ki}{\frac{3}{2}m^2} + \frac{4ki^2}{(\frac{3}{2}m^2)^2} \right] \sin (it + \epsilon).$$

Now, Hansen's expression for the elevation of the variable

above the fixed ecliptic at any point whose longitude is λ is of the form

$$-p \cos \lambda + q \sin \lambda,$$

where p and q are functions of t, expressed in series of powers of t.

Comparing this with Laplace's expression for the same quantity, we have

$$-p = \Sigma k \sin (it + \epsilon),$$

hence

$$-\frac{dp}{dt} = \Sigma ki \cos (it + \epsilon),$$

and

$$\frac{d^2p}{dt^2} = \Sigma ki^2 \sin (it + \epsilon);$$

similarly

$$q = \Sigma k \cos (it + \epsilon),$$

$$-\frac{dq}{dt} = \Sigma ki \sin (it + \epsilon),$$

$$-\frac{d^2q}{dt^2} = \Sigma ki^2 \cos (it + \epsilon).$$

Hence, by substituting for $k \sin (it+\epsilon)$, $k \cos (it+\epsilon)$, &c. in Laplace's expression for s_1, their values in terms of p, q and their differential coefficients, we find

$$s_1 = \sin \nu \left[-\frac{1}{\frac{3}{2}m^2} \frac{dp}{dt} - \frac{1}{(\frac{3}{2}m^2)^2} \frac{d^2q}{dt^2} \right] + \cos \nu \left[-\frac{1}{\frac{3}{2}m^2} \frac{dq}{dt} + \frac{1}{(\frac{3}{2}m^2)^2} \frac{d^2p}{dt^2} \right],$$

which exactly agrees with Hansen's expression in his *Darlegung*, p. 490,* except that Hansen's argument $f+\omega-\theta_1$ represents the longitude on the orbit, whereas Laplace's argument ν is the longitude on the ecliptic; but these two longitudes may be employed indifferently in terms of the order of small quantities to which the approximation is restricted.

Laplace remarks that $\frac{3}{2}m^2$ is at least 4,000 times greater than $2i$, and he therefore infers that the above value of s_1 may be

---

* In this expression $\frac{dp}{dt}$ is equivalent to $b+b't$ in Hansen, and $\frac{dq}{dt}$ is equivalent to $c+c't$.

Also Hansen's expression $n(\alpha+\eta)$, which denotes the mean motion of the Moon's node, is equivalent to $\frac{3}{2}m^2$ in Laplace, as the latter takes n, the Moon's mean motion, to be equal to unity.

neglected as insensible. If, however, the numerical values of the quantities denoted by k had been known to Laplace, he would have seen that some of those values are very considerable, exceeding one degree, and therefore that $\frac{1}{1000}$ of this amount is by no means to be neglected.

Finally, we will reduce Laplace's transformed expression to a form immediately comparable with our former results.

The velocity perpendicular to the ecliptic of a point in any arbitrary longitude L is represented in one system by

$$-\frac{dp}{dt}\cos \mathrm{L} + \frac{dq}{dt}\sin \mathrm{L},$$

and in the other system by

$$\omega \sin (\mathrm{L}-\mathrm{C}).$$

Hence

$$\frac{dp}{dt} = \omega \sin \mathrm{C},$$

and

$$\frac{dq}{dt} = \omega \cos \mathrm{C};$$

$$\frac{d^2p}{dt^2} = \frac{d\omega}{dt}\sin \mathrm{C} + \omega \frac{d\mathrm{C}}{dt}\cos \mathrm{C},$$

and

$$\frac{d^2q}{dt^2} = \frac{d\omega}{dt}\cos \mathrm{C} - \omega \frac{d\mathrm{C}}{dt}\sin \mathrm{C}$$

Hence, putting c for $\frac{3}{4}m^2$, and denoting the Moon's longitude by θ as before, instead of Laplace's ν, we have

$$\begin{aligned} s_1 = \quad & \sin\theta\left[-\frac{1}{c}\omega \sin \mathrm{C} - \frac{1}{c^2}\left(\frac{d\omega}{dt}\cos \mathrm{C} - \omega\frac{d\mathrm{C}}{dt}\sin \mathrm{C}\right)\right] \\ & + \cos\theta\left[-\frac{1}{c}\omega \cos \mathrm{C} + \frac{1}{c^2}\left(\frac{d\omega}{dt}\sin \mathrm{C} + \omega\frac{d\mathrm{C}}{dt}\cos \mathrm{C}\right)\right], \end{aligned}$$

or

$$\begin{aligned} s_1 &= -\frac{\omega}{c}\cos(\theta-\mathrm{C}) - \frac{1}{c^2}\frac{d\omega}{dt}\sin(\theta-\mathrm{C}) + \frac{\omega}{c^2}\frac{d\mathrm{C}}{dt}\cos(\theta-\mathrm{C}), \\ &= -\left(\frac{\omega}{c} - \frac{\omega}{c^2}\frac{d\mathrm{C}}{dt}\right)\cos(\theta-\mathrm{C}) - \frac{1}{c^2}\frac{d\omega}{dt}\sin(\theta-\mathrm{C}), \end{aligned}$$

which is in accordance with the remark made at the close of investigation I.

Observations of the Solar Eclipse of 1880, *December* 31, *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

The observations of this eclipse with the South-East Equatoreal were directed to the determination of the corrections to the Moon's R.A. and N.P.D. and to the semi-diameters of the Sun and Moon.

Putting

$d\alpha$ = correction to Moon's R.A.		$d\mathrm{A}$ = correction to Sun's R.A.	
$d\delta$ = " "	N.P.D.	$d\Delta$ = " "	N.P.D.
ds = " "	semidiam.	$d\mathrm{S}$ = " "	semidiam.,

the following arrangement of observations was drawn up, and the measures were made, by Mr. Christie, the declination microscopes being read by Mr. Criswick:—

(1) From $1^h\ 40^m$ to $1^h\ 55^m$ G.M.T., 10 differences of N.P.D. of N. and S. cusps, giving

$$(d\alpha - d\mathrm{A}) + \tfrac{1}{2}(d\delta - d\Delta) + ds + d\mathrm{S}.$$

(2) From $1^h\ 55^m$ to $2^h\ 15^m$ G.M.T., 10 differences of R.A. of p and f cusps, giving

$$d\alpha - d\mathrm{A} + ds + d\mathrm{S}.$$

(3) From $2^h\ 15^m$ to $2^h\ 40^m$ G.M.T., 13 differences of N.P.D. of S. limbs of Moon and Sun, giving

$$-(d\delta - d\Delta) - ds + d\mathrm{S}.$$

(4) From $2^h\ 40^m$ to $3^h\ 0^m$ G.M.T., 11 differences of N.P.D. of p and f cusps, giving

$$d\alpha - d\mathrm{A}.$$

(5) From $3^h\ 0^m$ to $3^h\ 25^m$ G.M.T., 12 differences of R.A. of p and f cusps, giving

$$-(d\alpha - d\mathrm{A}) + (d\delta - d\Delta) + ds + d\mathrm{S}.$$

Five resulting equations were found by taking the sums of the constituent equations in each group, obtained by a comparison of the measured with the tabular quantity as affected by the symbolical corrections $d\alpha - d\mathrm{A}$, $d\delta - d\Delta$, ds, and $d\mathrm{S}$:—

$-322''.79$ =	$+18.094(d\alpha - d\mathrm{A})$	$+11.362(d\delta - d\Delta)$	$+24.570\ ds$	$+24.596\ d\mathrm{S}$
-168.73 =	$+9.797$	-0.821	$+10.163$	$+10.512$
$+16.04$ =		-13.000	-13.000	$+13.000$
-159.59 =	$+11.789$	-1.016	-0.632	-0.604
$+100.18$ =	-10.376	$+16.483$	$+22.997$	$+23.471$

These being treated by the method of least squares gave the following four normal equations:—

$$
\begin{array}{rrrrr}
-10414''46 = & +670\,02(d\alpha - d\mathrm{A}) + & 14\cdot54(d\delta - d\Delta) + & 298\cdot07\,ds + & 297\cdot37\,d\mathrm{S} \\
-\ 7056\cdot80 = & +297\cdot37 & +489\cdot32 & +1082\cdot30 & +1435\cdot72 \\
-\ 7449\cdot57 = & +298\cdot07 & +819\cdot52 & +1405\cdot23 & +1082\cdot30 \\
-\ 1924\cdot13 = & +\ 14\cdot54 & +571\cdot49 & +\ 819\cdot52 & +\ 489\cdot32
\end{array}
$$

the solution of which is

$$
\begin{aligned}
d\alpha - d\mathrm{A} &= -14''23 \\
d\delta - d\Delta &= +\ 1\cdot17 \\
ds &= -\ 2\cdot72 \\
d\mathrm{S} &= -\ 0\cdot31
\end{aligned}
$$

From observations of the Sun with the Transit Circle on 1880 Dec. 9, 10, 22, 24, and 1881 Jan. 5, 6, 7, 10, 12, 15, the corrections to the Sun's tabular place are found to be

$$d\mathrm{A} = -1''74; \quad d\Delta = -0''44.$$

Combining this result with that obtained from the Equatoreal observations during the eclipse, we obtain for the corrections to the tabular place of the Moon in R.A. and N.P.D. :—

$$
\begin{aligned}
d\alpha &= -15''97 = -1^{s}06 \\
d\delta &= +\ 0\cdot73
\end{aligned}
$$

In reducing the observations, Bessel's Refractions of the *Tabulæ Regiomontanæ*, with Main's corrections below Z.D. 82°, have been used. Notwithstanding the low altitude of the Sun (3° 46′ at the last observation), the individual measures are very accordant, and there is no appreciable uncertainty in the refraction correction, which in no case exceeds 9″ in its differential effect on the two limbs or cusps observed.

Royal Observatory, Greenwich:
1881, *June* 8.

On the Moon's Photographic Diameter and on the Applicability of Celestial Photography to Accurate Measurement. By Prof. C. Pritchard.

(*Abstract.*)

The object of this communication is to investigate the amount of reliability assignable to Photography, as applied to the Moon; the enquiry being preliminary to a discussion of the nutation of the Moon's rotational axis, commonly called the Physical Libration, as obtained from measures made on Lunar Photographs.

1. In the first instance the instruments used are described and their scales or constants are determined.

2. The identity of the photographic and the ordinary telescopic image of the Moon for purposes of measurement is demonstrated.

3. The effects of inequalities on the Moon's limbs is shown to be inappreciable. This is demonstrated by measures taken from Triesnecker B or Ptolemy A to 74 points in succession, and without selection.

4. The effect of temperature on the general result is approximately determined.

5. The effect of the time of exposure of the photographic plate is ascertained.

6. Investigations are made respecting the effects of the Moon's altitude, and of the particular limb to points on which the measures are made.

7. The effect of any possible shrinkage of the photographic film is investigated.

8. The result of the entire set of enquiries shows that the photographic method is at least equal in point of accuracy to direct telescopic measures made on the actual disk of the Moon.

On the Comparison of the Computed and Observed Motions of the Components of the Binary System ξ Ursæ Majoris. By Prof. C. Pritchard.

About three years ago, in the first Fasciculus of the astronomical work done at the Oxford University Observatory, I published a brief account of a method which I had devised for the investigation of the relative orbits of binary stars. The method certainly offers peculiar facilities for such computations, and it seemed to me to hold out considerable promise at the time, especially as, owing to the wide discrepancies of ancient, and even of some modern, observations, abstruse and complicated mathematical processes appeared practically inapplicable. The method was, among other double stars, applied in particular to the case of *ξ Ursæ Majoris*, of which the elements are recorded in the Fasciculus referred to.

It is somewhat remarkable that Dr. Düner at Lund computed also an orbit for this system about the same time that the foregoing was computed at Oxford; that able astronomer employing an elaborate method wholly different from that devised by myself, yet an almost identity of elements was the result: and no doubt can now exist, that of *ξ Ursæ*, at least, the orbit is very approximately settled. The two sets of elements are as follows:—

	Oxford.	Düner.
Date of Perihelion Passage	1875·26	1875·29
Period of Revolution	60·80 yrs.	60·79 yrs.
Position of Node	100° 13′	101° 30′
Inclination of Orbit	56° 40′	56° 54′
Angle between Axis Major and Line of Nodes	235° 0′	234° 18′
Excentricity of Orbit	0·4159	0·3952
Semi-Axis Major	2″·580	2″·547

The following Table I. contains a comparison of the theoretical determination with observed facts during the last six years since the orbit was computed. The observed places herein given have been taken from the interpolation curve, drawn amongst the individual measures that have been hitherto published.

I.

Date.	Position Angle from Interpolation Curve.	Computed Position Angle (Oxford Elements).	Computed Position Angle (Dilner's Elements).	C—O Oxford.	C—O Dilner.	Distance from Interpolation Curve.	Computed Distance (Oxford Elements).	Computed Distance (Dilner's Elements).	C—O Oxford	C—O Dilner.
	°	°	°	°	°	"	"	"	"	"
1875·0	322·0	322·8	323·3	+0·8	+1·3	1·03	1·05	1·08	+0·02	+0·05
1876·0	309·1	307·7	309·1	−1·4	0·0	1·37	1·24	1·26	−0·13	−0·11
1877·0	298·5	296·6	298·6	−1·9	+0·1	1·54	1·43	1·44	−0·11	−0·10
1878·0	289·0	288·0	290·2	−1·0	+1·2	1·68	1·59	1·60	−0·09	−0·08
1879·0	281·0	280·8	283·2	−0·2	+2·2	1·78	1·71	1·72	−0·07	−0·06
1880·0	274·9	274·4	277·1	−0·5	+2·2	1·87	1·80	1·80	−0·07	−0·07
1881·0	269·0	268·5	271·3	−0·5	+2·3	1·88	1·85	1·85	−0·03	−0·03

The curve for the last few years is based on the observations given in Table II., in which may be seen the amount of divergence of the computed places from those measured by individual observers. For the earlier years, extending from 1819 to 1878, the comparisons of the observed with the computed places, founded on the Oxford Elements, are given in the Fasciculus referred to. The comparison of Dr. Düner's places will be found on page 197 of *Mesures Micrométriques d'Étoiles doubles* (Lund, 1876).

II.

Comparison with Oxford Elements.

Date.	Observed Angle.	Computed Angle.	Diff. C−O.	Observed Distance.	Computed Distance.	Diff. C−O.	Observer.
	°	°	°	"	"	"	
1877·26	294·9	294·2	−0·7	1·43	1·48	+0·05	Dembowski
77·34	293·9	293·5	−0·4	1·46	1·49	+0·03	Plummer
77·40	294·8	293·3	−1·5	1·52	1·50	−0·02	Wilson
77·47	294·2	292·3	−1·9	1·49	1·51	+0·02	Jedrzejewicz
78·25	286·9	286·1	−0·8	1·62	1·63	+0·01	Plummer
79·18	279·3	279·6	+0·3	1·70	1·73	+0·03	Plummer
79·18	278·1	279·6	+1·5				Gledhill
80·19	273·7	273·3	−0·4	1·84	1·81	−0·03	Plummer
80·20	272·0	273·2	+1·2	1·81	1·81	0·00	Gledhill
80·48	272·0	271·5	−0·5	1·81	1·83	+0·02	Jedrzejewicz
81·25	268·1	267·1	−1·0	1·84	1·86	+0·02	Plummer

Comparison with Dr. Düner's Elements.

1877·26	294·9	296·2	+1·3	1·43	1·49	+0·06	Dembowski
77·34	293·9	295·5	+1·6	1·46	1·50	+0·04	Oxford
77·40	294·8	295·0	+0·2	1·52	1·51	−0·01	Wilson
77·47	294·2	294·4	+0·2	1·49	1·52	+0·03	Jedrzejewicz
78·25	286·9	288·4	+1·5	1·62	1·63	+0·01	Oxford
79·18	279·3	282·1	+2·8	1·70	1·74	+0·04	Oxford
79·18	278·1	282·1	+4·0				Gledhill
80·19	273·7	276·0	+2·3	1·84	1·82	−0·02	Oxford
80·20	272·0	276·0	+4·0	1·81	1·82	+0·01	Gledhill
80·48	272·0	274·3	+2·3	1·81	1·83	+0·02	Jedrzejewicz
81·25	268·1	270·0	+1·9	1·84	1·86	+0·02	Oxford

In 1825, Sir John Herschel remarks that this star merits "constant and careful observation." Now that so close an approximation has been made to the representation of the actual facts, the interest attaching to such questions has unquestionably increased.

Before concluding this communication I think it my duty to the Society to record the great practical assistance which I have derived from Sir John Herschel's Reference Catalogue of 10,000 double stars, published in vol. xl. of their *Memoirs*. The chief utility of such Catalogues relates to their furnishing materials for the computations of orbits, and in that volume there is an asterisk or distinguishing mark placed against every binary star of which an orbit has ever yet been computed, or, for some years to come, is likely to be computed. The asterisk indicates that Sir John Herschel bequeathed to the Society a collection of papers, accessible to astronomers and containing a well-arranged and complete account of every observation he could find of the double star in question. It extends to every star in W. Struve's *Mensuræ Micrometricæ*, and forms a noble bequest and invaluable repertory. At some future time there may be a few other double stars, of which it may be worth the trouble and expense of the Society to collect a complete series of observations; but at present W. Struve's binaries are far from exhausted. In reference to this intrinsically valuable Catalogue, the last work of the great astronomer's declining years, I may mention that, on comparing the new edition of *Admiral Smyth's Cycle* with the Reference Catalogue in vol. xi., I found (with one solitary and very intelligible exception) every double star that is contained in the Cycle, recorded and referred to also in the Catalogue, up to the date of its completion.

Oxford:
1881, *June* 8.

The Planetary Nebula in Cygnus (*R.A.* $21^h\ 2^m$, *Decl.* 47° 22′).

By S. W. Burnham, Esq.

Astronomers interested in nebulæ seem to have overlooked the fact that the Planetary Nebula found by Dr. Copeland at the Dunecht Observatory last November was discovered a good many years ago. I picked it up July 6, 1873, with the 6-inch refractor, and described it in *Monthly Notices* of the Royal Astronomical Society for December 1873 (vol. xxxiv. p. 71) as "a very remarkable and curious double or elongated planetary (?) nebula. It is close to a 9·3 mag. star, *Arg.* (47°)3289. P=245°; D=30″. It bears magnifying well, and the elongation could not be seen with less than 400. With a large aperture it would probably be a very interesting object."

Subsequently, in 1878, it was examined with the 18½-inch

refractor of the Dearborn Observatory, and the nuclei measured with reference to each other, and to the 9·3 mag. star. These observations will be found in *Memoirs* of the R.A.S. vol. xliv. pp. 78, 157. It will be noticed that my measures of the nuclei disagree entirely with those of Dr. Copeland (*Urania*, Jan. 1881).

Burnham	P = 88°·5	D = 2″·6	1878·47
Copeland	71·8	8·0	1880·91

I cannot but think there is considerable error from some source in the last distance; for between 1873, when, as stated above, a high power was necessary to show the double character of the nebula, and 1878, when my measures were made, there was no apparent change whatever; and I am quite confident I looked at it at least once in 1880 and found its appearance substantially the same. A re-examination, however, will at once show what the real or approximate distance is.

Whatever the merits or advantages of the spectroscopic plan for the discovery of nebulæ may be generally, certainly no new method is required for this particular object; for any good refractor of four or five inches aperture is quite sufficient to detect it at once with an ordinary eyepiece.

Chicago:
1881, *March* 8.

The Nebula near Merope. By G. W. Hough, Esq., and S. W. Burnham, Esq.

The observations already made in regard to Tempel's Nebula in the *Pleiades*, although very numerous, are nevertheless so discordant that additional facts may be of interest. In Herschel's Catalogue the description is as follows: exceedingly interesting object; bright; very large; irregular figure; variable. From this description it would be inferred that it was a comparatively easy object.

The casual examination of *Merope* and its immediate neighbourhood on a number of nights during 1879 and 1880 with the 18½-inch Clark Refractor, with powers 120 and upwards, failed to show any nebulosity whatever.

The recent very positive statement of Tempel in the *Monthly Notices*, vol. xl. No. 9, in which the Nebula is so sharply delineated, led us to make a thorough examination of *Merope* with the Chicago Refractor, using various powers and apertures, to satisfy ourselves with regard to its existence.

In order that the circumstances affecting its visibility should be the most favourable, only first-class observing nights were chosen for making these examinations.

The following brief notes give the results of our work in this direction.

Nov. 29, 1880. Seeing good. Comet eye-piece, negative. Power 60; object-glass 12 inches, and full aperture.

When *Merope* was placed near the edge or outside the field, a faint glow was seen, south and preceding, in the general direction corresponding to Tempel's drawing, but nothing which would ordinarily be regarded as a nebula.

Dec. 2, 1880. Seeing excellent. The gauze ring of *Saturn* very sharply defined. Positive eye-piece. Power 120.

When *Merope* was placed near the edge of the field a faint glow was seen similar to that observed on Nov. 29th. A narrow strip of tinfoil was then placed across the field to cut off the direct light of *Merope*, when the glow disappeared, and no trace of nebulosity was visible.

Jan. 30, 1881. Seeing good. *Jupiter* and *Saturn* would bear a power of 638. Positive eye-piece. Power 120.

With a clear field, no glow or nebulosity could be seen. An occulting-bar was then used to cut off the light of *Merope*, with the same result.

March 22, 1881. Seeing good. Comet-eye-piece. Powers 60 and upwards. No trace of nebula was visible. Immediately after making this examination the telescope was set for the following faint Nebulæ, all of which proved to be comparatively easy objects: Dreyer and Herschel's, Nos. 5381, 5388, 5399, 5400 5415, 5416, 1661.

The observation on November 29 led us to conclude that possibly the faint glow was due to nebulous matter, but the subsequent observation, in which an occulting-bar was used, proved conclusively that it proceeded from *Merope*.

With higher powers, no trace of nebula was ever seen in the neighbourhood of *Merope*.

So many observers, however, with various kinds of telescopes, have obviously seen something *near* this locality, that the question is one of more than ordinary interest, and the negative testimony of any one instrument should have but little weight, provided the positive evidence is sufficiently concordant. Whether, however, observers have seen Tempel's Nebula or something else will best be ascertained by comparing all observations with Tempel's description and drawing. A Nebula seen in any other part of the *Pleiades* cannot, therefore, be regarded as the object under discussion.

Tempel says: "It is now ascertained beyond question that the Nebula exists. It has been observed with the great Washington Refractor, and again with the Reflector of Lord Rosse."

In how far this statement is warranted may be ascertained from the following facts :—

"Washington Observations," 1874, p. 75.

"Tempel's Nebula in the *Pleiades* has been viewed occasionally."

"Wash. Obs." 1878.

"1875, Oct. 25. Power 158. Observer, Holden.

"*Merope* is a nebulous star, with a dark space on its *preceding north* and *following* sides. To the *south* it is joined by nebulosity to the main body of nebulosity above. This is not uniform in brightness, but cut up into portions by dark channels, several of them *north*, and following *Merope*. The nebulosity extends north [south] of this star some distance, some 6′ or 7′ at least, and perhaps more, as this point was not looked to. With 400, *Merope* is seen nebulous, but the rest is lost."

"Comet-seeker: power [?].

"I have drawn a dotted line on Tempel's map (pub. "R. O. di Milano," No. 5, plate ii.) where I see the boundary. I am sure it is further extended to the south and west than Tempel has it. No nebulosity seen north of *Merope*."

"1875, Oct. 29. Positive; 173.

"*Merope* looks nebulous to a distance of, say, 60″ following; then comes a dark streak, and then a narrow nebulous wisp (pos. 150°±est.) which passes through a star 14 mag., 20^s following *Merope*. This 14 mag. star is the first star following *Merope* in the same declination (approximately).

"I see nothing of Wolf's 2^d nucleus, 7^s following *Merope*."

"Observatory," vol i. p. 370. Reference to Tempel's Nebula in the *Pleiades*. It is not stated, however, what was seen with Lord Rosse's great Reflector.

Monthly Notices, vol. xl. No. 6. Mr. Common's 3-ft. Reflector. Three nebulous patches are shown, all outside of Tempel's boundary, and entirely different objects from Tempel's Nebula.

Monthly Notices, vol. xl. No. 2. Maxwell Hall. 4-inch Refractor; power 100. Drawing very similar to the one given by Tempel.

Schiaparelli, Feb. 25, 1875. "He noted it to extend from *Merope* beyond *Electra*, and as far as *Celeno*."

Monthly Notices, vol. xl. No. 5. Dr. C. Wolf. 12-inch Refractor.

"Elle se prolonge vers *Electre* et au-delà de *Cœleno*, comme l'a dit Schiaparelli."

Dr. Engelman. Circular patch of light 11′ from *Merope* in the direction of Tempel's Nebula.

A comparison of the observations of these various authorities leaves the position, extent, and boundary of the Nebula very uncertain. It seems clear, however, that the great telescopes do not locate it in the position assigned by Tempel.

The angle of position of the axis of the Nebula with reference to *Merope*, as seen by different observers, appears to be approximately as follows :—

Tempel, Maxwell Hall, Engelman	205°
Wolf and Schiaparelli	315°
Common...	20°, 135°
Holden	150°, 25°, 205°

In view of such great discrepancies by observers with first-class telescopes, we think it may with great propriety be asked whether these various appearances are due to real matter, or are simply an illusion.

We are strongly inclined to the opinion that the phenomena in question are due to the glow proceeding from *Merope* and neighbouring stars. With a small telescope and low power we should have to contend with the light from numerous stars in different parts of the field. As the size of the telescope is increased, the field will be diminished, and a less number of stars will be included, which might materially modify the phenomena.

In our observations of Nov. 29 and Dec. 2, the glow seemed to have a definite outline, on the north and following sides, such as might be produced by real matter; but so soon as an occulting-bar was introduced to cut off the direct light of *Merope*, the whole phenomenon disappeared : whereas, had it been due to real matter, it should have been more conspicuous when the occulting-bar was used.

Until, therefore, the observations by different observers and telescopes are better reconciled than at present—not to mention the prominent astronomers who have failed to detect any nebula—we think there is just ground to doubt the existence of any real matter in the position assigned for this Nebula.

Dearborn Observatory :
1881, March 29.

Ephemeris for finding the Positions of the Satellite of Neptune, 1881–82. By A. Marth, Esq.

P, angle of position of the minor axis of the satellite's apparent orbit, in the direction of superior conjunction.

a, *b*, major and minor semi-axis of the apparent orbit.

Long.=longitude of the satellite in its orbit, reckoned from the point which is in superior conjunction with the planet.

G. Noon.	P.	*a*	*b*	log. *a*	log. *b*	Long.	Diff.
1881.	°	″	″			°	°
Sept. 6	315·72	16·68	6·80	1·2222	0·8323	99·65	612·50
16	315·64	16·76	6·82	·2243	·8336	352·15	·44
26	315·53	16·83	6·83	·2261	·8342	244·59	·40
Oct. 6	315·39	16·89	6·83	1·2276	0·8342	136·99	·36
16	315·23	16·93	6·81	·2288	·8334	29·35	·33
26	315·06	16·96	6·79	·2295	·8320	281·68	·31
Nov. 5	314·89	16·97	6·76	1·2298	0·8301	173·99	·29
15	314·71	16·97	6·72	·2296	·8275	66·28	·29
25	314·54	16·94	6·68	·2290	·8246	318·57	·31
Dec. 5	314·38	16·90	6·63	1·2279	0·8213	210·88	·32
15	314·25	16·84	6·57	·2261	·8178	103·20	·34
25	314·14	16·77	6·52	·2246	·8143	355·56	·39
1882.							
Jan. 4	314·06	16·69	6·47	1·2225	0·8109	247·95	·44
14	314·01	16·60	6·42	·2202	·8077	140·39	·50
24	314·00	16·51	6·38	·2177	·8049	32·89	·54
Feb. 3	314·02	16·41	6·35	1·2152	0·8025	285·43	·60
13	314·07	16·32	6·32	·2127	·8006	178·03	·65
23	314·16	16·23	6·30	·2103	·7993	70·68	612·71
Mar. 5	314·29	16·14	6·29	1.2080	0·7986	323·39	

These values are to be interpolated for the times for which the apparent places of the satellite are required, and the position-angles p and distances s are then found by

$$s \sin (P-p) = a \sin \text{long.}$$
$$s \cos (P-p) = b \cos \text{long.}$$

The satellite moves in the direction of decreasing position-angles, and will be at its greatest elongations ("nf." in posit. P+90° and distance *a*, "sf." in posit. P−90°) and at its conjunctions ("sup." in posit. P and distance *b*, "inf." in posit. P−180°) at the following hours, Gr. M. T.:—

	"sp." Elong. h		"inf." Conj. h		"nf." Elong. h		"sup." Conj. h
1881, Sept.	5	20·2	Sept. 7	7·5	Sept. 8	18·7	Sept. 10	6·0
	11	17·3	13	4·5	14	15·8	16	3·1
	17	14·3	19	1·6	20	12·9	22	0·2
	23	11·4	24	22·7	26	10·0	27	21·2
	29	8·5	30	19·8	Oct. 2	7·0	Oct. 3	18·3
Oct.	5	5·6	Oct. 6	16·9	8	4·1	9	15·4
	11	2·7	12	13·9	14	1·2	15	12·5
	16	23·8	18	11·0	19	22·3	21	9·6
	22	20·9	24	8·1	25	19·4	27	6·7
	28	18·0	30	5·3	31	16·5	Nov. 2	3·8
Nov.	3	15·1	Nov. 5	2·4	Nov. 6	13·6	8	0·9
	9	12·2	10	23·5	12	10·7	13	22·0
	15	9·3	16	20·6	18	7·9	19	19·1
	21	6·4	22	17·7	24	5·0	25	16·2
	27	3·5	28	14·8	30	2·1	Dec. 1	13·3
Dec.	3	0·6	Dec. 4	11·9	Dec. 5	23·2	7	10·4
	8	21·7	10	9·0	11	20·3	13	7·5
	14	18·8	16	6·1	17	17·4	19	4·6
	20	15·9	22	3·2	23	14·5	25	1·7
	26	13·0	28	0·3	29	11·6	30	22·8
1882, Jan.	1	10·1	Jan. 2	21·4	Jan. 4	8·6	Jan. 5	19·9
	7	7·2	8	18·5	10	5·7	11	17·0
	13	4·3	14	15·5	16	2·8	17	14·1
	19	1·3	20	12·6	21	23·8	23	11·1
	24	22·4	26	9·6	27	20·9	29	8·2
	30	19·4	Feb. 1	6·7	Feb. 2	18·0	Feb. 4	5·2
Feb.	5	16·5	7	3·7	8	15·0	10	2·3
	11	13·5	13	0·8	14	12·0	15	23·3
	17	10·5	18	21·8	20	9·1	21	20·3
	23	7·6	24	18·8	26	6·1	27	17·3
March	1	4·6	March 2	15·8	March 4	3·1	March 5	14·3

In reference to his Paper, "Telegraphic Determination of the Longitude of Shanghai," *Monthly Notices*, December No. *ante*, pp. 64–67, Dr. L. S. Little writes:—

"The object and public use of the determination of the longitude of the Observatory was, that by it I determined the longitude of the British Consular Flagstaff, which is the point of departure of the charts, &c. This point is $1^{s}\cdot02$ E. of Observatory, and the only point in China determined telegraphically"

Observations of the Companion of Sirius, made at the U.S. Naval Observatory, Washington.

(*Communicated by Rear-Admiral John Rodgers, U.S.N., Superintendent.*)

Date.	Sid. Time.	p.	s.	Weight.	Observer.
	h	°	″		
1881, Jan. 28	7·5	43·1		3	Hn.
Feb. 17	6·9	46·0	10·63	3	F.
19	6·5	46·4	9·96	3	F.
25	8·8	43·5	10·83	4	Hn.
March 7	7·0	48·4	10·02	3	F.
10	7·0	46·2	10·01	2	F.
11	6·5	46·0	10·02	2	F.
14	7·7	48·3	10·01	2	F.
26	6·9	45·3	9·99	2	H.
27	7·0	44·8	10·17	2	H.
28	6·9	44·2	9·88	3	H.
April 7	8·0	44·5	10·01	2	H.
10	8·2	46·3	10·00	2	H.
11	8·5	46·7	9·96	3	H.

Results.

	p.	s.	Observers.
	°	″	
1881·117	43·30	10·83	E. S. Holden.
1881·174	46·88	10·108	E. Frisby.
1881·256	45·30	10·002	A. Hall.

Errata.

P. 334, last line but one, last column, *for* + 1·59 *read* + 1·95.

„ 357, ζ *Aræ*, in column for Number of Observations at Lower Transit, *insert* 6, and three lines lower down, *for* Star' *read* Star's.

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XLI. SUPPLEMENTARY NOTICE. No. 9.

On the Value of the Parallactic Inequality. By James Campbell, Esq., and E. Neison, Esq.

The Note by Mr. Stone in the *Monthly Notices* for June seems to call for a brief explanation from us, which we beg now to lay before the Society.

We consider that our last communication to the Society (*Monthly Notices*, March 1881) contains a satisfactory proof of the validity of the correction for the effects of the irradiation at the lunar limb which has been employed by us in our investigation. Mr. Stone would still seem to be of opinion that such proof is wanting, and merely repeats two objections shown by us in our last communication to be due to a misunderstanding and therefore to be quite untenable, as if he had overlooked this portion. We need add nothing further, therefore, on this point.

With regard to the third section of Mr. Stone's Note, the only point which seems to call for any reply is the objection he has urged that we have employed an erroneous value of the semi-diameter of the Moon in the reduction of the observations for the period 1851–1858. It appears to us that Mr. Stone's objection can be summed up as follows: These observations have been reduced by us with Adams' semi-diameter plus the correction ($-0''.18$), whereas we ought to have used, as before, Hansen's value plus the correction ($+0''.24$),* and consequently our results ought to be increased by $+0.94$, or the difference between the *mean* values of the semi-diameter according to Hansen and Adams when similarly corrected.

We dissent entirely from this conclusion.

In determining the value of the parallactic inequality it is

* By some inadvertence Mr. Stone calls it $+0''.29$.

essential that the observations should be reduced with the observed value of the lunar semi-diameter, and if the observed value be employed it is quite immaterial what the tabular value may be. In all our results we have employed the observed value, and if no mention has been made of a correction for converting the tabular value (Adams') into the observed value, it is solely because for this particular period the observed and tabular semi-diameters coincided in value. When, however, we come to compare Adams' value for the semi-diameter founded on Adams' corrected version of Burckhardt's tabular parallax with Hansen's value for the semi-diameter founded on his own tables, it is found that a small correction ($-0''.18$) * must be employed. It follows, therefore, that our results rest directly on the observed semi-diameters, and do not, as Mr. Stone supposes, require a correction for the difference between the *mean* semi-diameters of Adams and Hansen.

Mr. Stone seems to think that the same mean correction to the semi-diameter should be employed at all epochs. From this we dissent, for as the observers change, even if it be merely in the relative frequency of their observations, as they become more experienced, and as the imperfections in the tables develop themselves, so will vary the correction necessary to reduce the tabular to the observed semi-diameter.

The difficulties imagined by Mr. Stone in the last paragraph of his paper do not seem weighty: the constant difference of $+1''.60$ between the results prior and subsequent to the change of instruments in 1816 is exactly what might be anticipated by the change of aperture, and its reality is clearly shown by the observations themselves. It is exactly analogous to the corresponding change which occurred in 1851, when a similar change of instruments occurred.

Mr. Stone also lays special stress on the fact that the results for the years 1846–1851 (for an examination of the results for the separate years will show that it is only these years which are discordant) do not agree with those made at earlier periods. This admitted discordance we were disposed to attribute to some change of observers at this epoch. Further examination confirms this; for it shows that towards the end of 1845 such a change did occur. For many years previous to this date the regular observers had been Mr. Henry and Mr. Thomas Ellis, but at this date Mr. Rogerson replaced Mr. Ellis. In 1846 Mr. Dunkin replaced Mr. Rogerson, but towards the middle of the year Mr. Dunkin again gave place to Mr. Rogerson. Towards the end of 1848, the practice of all four assistants taking regular part in the observations was introduced, and continued until quite lately. No very abundant material exists for comparing the observations of Messrs. Rogerson and Dunkin with those of Mr. Ellis through those of Mr. Henry, but a reduction of the

* The value found from our own comparison was $-0''.15$; that used seems to have been deduced in a similar manner at Greenwich.

only existing data clearly shows that whilst the diameter of the Moon according to Messrs. Rogerson and Dunkin agreed with that according to Mr. Henry, the diameter according to Mr. Ellis was entirely different, he having a very great personality (amounting to nearly 3″·00) in this respect. Therefore, to render the results for the years 1846–1851 comparable with those for the earlier years, the effect of this personality must be considered; and it results that the parallactic inequality for these years must be increased by nearly 2″·00. With this correction the discordance vanishes.

We submit, therefore, that these objections which have been made to the validity of the results obtained by us in our investigations have been shown to be illusory, and though Mr. Stone may not see this just at present, we are confident that, with the evidence already published, this is a view which will be confirmed by those astronomers who are properly versed in these matters.

Note on Mr. Gill's Ascension Observations. By G. F. Hardy, Esq.

In his Paper summarising the results of the observations made at Ascension with a view to determine the solar parallax, Mr. Gill gives an expression for the error in R.A. of the tabular places of *Mars*, which includes a term depending upon twice the mean longitude of the Moon. Mr. Gill seems to consider the existence of this term somewhat doubtful, or at least as unexplained. It will be remembered, however, that the geocentric places of the planets equally with the Sun are affected by the revolution of the Earth round the common centre of gravity of itself and its satellite, the magnitude of the resulting irregularity depending upon the apparent angular diameter of the small orbit thus described, as seen from the planet in question. The *Nautical Almanack* places of *Mars* being from Leverrier's tables, we may suppose that the magnitude of this irregularity was determined on the basis of Leverrier's estimate of 8″·95 for the value of the solar parallax. As this is now known to be too large, it follows that the magnitude of the irregularity alluded to is also over-estimated in the *Nautical Almanack* tables, and that the observed places will be alternately behind and in front of the tabular places of the planet. It is, of course, only owing to the fact that the observations extend through a comparatively short period that the necessary correction can be fairly represented by a term having a constant coefficient, as its value does not actually depend upon the Moon's longitude alone, but on the difference of the longitude of the Moon and planet, and further, on the ratio of their respective parallaxes. Assuming that the term given by Mr. Gill represents with sufficient nearness the correction required, and that we may assume a constant average parallax for the planet during the period through which the

observations extended, it is easy to calculate approximately the correction required by Leverrier's value of the parallax. Assuming the average tabular parallax of *Mars* during the Ascension observations to have been 21″, and taking Mr. Gill's constant ″·376 as the correction to be deducted from this value, we have for the correction to the Sun's tabular parallax of 8″·95,

$$-\frac{″·376 \times 8·95}{21·00} = -″·16.$$

Thus, we get 8″·95 − ″·16 = 8″·79, agreeing extremely well with the general result of Mr. Gill's observations.

I have for some time thought that an extension of the method employed by Leverrier in the case of the Sun to determine the parallax of some of the nearer planets, would probably be attended with success, and the result brought out incidentally by Mr. Gill seems to support this view. One of the main difficulties would be the accurate determination of the Moon's mass. Probably, sufficiently numerous and exact observations exist to determine the lunar parallax to within $\frac{1}{100000}$th of its actual value, *i.e.* to about ″·03, while the length of the seconds pendulum may be considered as already known with an equal exactitude; these two data should therefore give us the value of the Moon's mass to within the $\frac{1}{1000}$th part of the truth. As respects other methods of determining the solar parallax, it appears to me that something more might probably be accomplished by a careful analysis of existing observations of *Jupiter's* satellites, treating the diameters of the planet and of the satellites as unknown quantities to be determined from the eclipse observations, and taking into account the possible effect of the refraction of the Jovian atmosphere (which may be considerable) on the times of disappearance and reappearance.

Stroud Green, N.,
1881, *July* 13.

On the Determination of the Moon's Libration.

By A. Marth, Esq.

In the method which has been adopted in several investigations of the Moon's libration, the apparent position of a spot referred to the centre of the Moon's disk and to the circle of declination is deduced from the measured distances and position-angles of a number of points of the Moon's limb in reference to the spot; from the apparent coordinates thus found the selenocentric right ascension and declination of the spot are deduced, and these are converted into ecliptical longitude and latitude. Comparison is then made with the selenocentric longitude and

latitude computed from approximate values of the spot's selenographical coordinates and of the inclination of the lunar equator to the ecliptic, and the equations are found which exhibit the effect of corrections of these values and of the terms due to physical libration.

It appears preferable to follow another course; to proceed from assumed selenographical coordinates of the spot to its apparent coordinates referred to the centre of the Moon's disk and the direction of the Moon's axis, to determine the corrections of these coordinates which best satisfy the measurements, and to form the equations between the corrections and the effect of the Moon's physical libration, &c., upon these coordinates.

Let I denote the assumed inclination of the plane of the lunar equator to the ecliptic; $\Omega + 180°$ the longitude of its ascending node, which coincides with the descending node of the orbit; J its inclination to the plane of the Earth's equator; N the right ascension of the ascending node. If, further, l_0 is the mean longitude of the Moon in its orbit and $M + 180°$ the arc of the lunar equator between equator and ecliptic, let $l_0 - \quad + M + N$ be denoted by a_0. The departure of the first or zero meridian, from which the selenographical longitudes are reckoned, from the ascending node $\Omega + 180°$ of the lunar equator on the ecliptic, is, in consequence of the equality of the periods of rotation and mean revolution, $= l_0 - \Omega$, and therefore its departure from the ascending node of the lunar equator on the Earth's equator

$$= l_0 - \Omega + M + 180° = a_0 - N + 180°,$$

so that the angle a_0 is in reference to the equator analogous to l_0 in reference to the ecliptic.

Ephemerides of the position of the lunar equator referred to that of the Earth have been given in the *Berliner Jahrbuch* since 1839, and in the *Nautical Almanac* since 1867, the notation and formulæ adopted being those employed by Encke in his paper "Ueber die selenocentrischen Constanten bei den Sternbedeckungen und die Berechnung der Libration des Mondes, nebst Tafeln," in the *Berliner Jahrbuch* for 1843. Instead of the arc of the lunar equator between the Earth's equator and the ecliptic, therein given, it will be found far more convenient to have the value of a_0, which is in Encke's notation

$$= l_0 - \Omega + 180° + \Delta + \Omega'.$$

For the present purpose the Ephemerides will have to be computed rather more accurately, l_0 and Ω being duly referred to the true equinox. Since, moreover, other formulæ will be found preferable to Encke's, there would be no advantage in retaining his designations i, Δ and Ω', which are replaced by the letters J, $M + 180°$ and N in the present paper.

The values of a_0, N and J may be deduced from those of l_0,

Ω, I, and of the apparent obliquity ϵ of the ecliptic by means of the formulæ

$$\tan \eta = -\tan \mathrm{I} \cos \Omega,$$
$$\tan \mathrm{N} = -\tan \mathrm{I} \sin \Omega \cdot \frac{\cos \eta}{\sin (\epsilon + \eta)},$$
$$\tan \mathrm{J} = \tan (\epsilon + \eta) \sec \mathrm{N},$$
$$\sin \tfrac{1}{2} (\alpha_0 - l_0) = -\frac{\sin \tfrac{1}{2} \mathrm{I} \sin \tfrac{1}{2} \epsilon}{\cos \tfrac{1}{2} \mathrm{J}} \cdot \sin \Omega.$$

The next proceeding is the computation of the position-angle P of the Moon's axis, and of the selenographical longitude Λ and latitude B of the centre of the Moon's disk as seen by the observer. Let α' and δ' be the apparent right ascension and declination of the Moon's centre, which are derived from the geocentric right ascension and declination α and δ, if θ is the sidereal time, by

$$r' \cos \delta' \sin (\alpha' - \alpha) = -\rho' \sin (\theta - \alpha),$$
$$r' \cos \delta' \cos (\alpha' - \alpha) = r \cos \delta - \rho' \cos (\theta - \alpha),$$
$$r' \sin \delta' = r \sin \delta - \rho'',$$

or by equivalent formulæ, in which $r (= \frac{1}{\sin \pi})$ and r' are the linear distances of the Moon's centre from the Earth's centre and from the observer, and ρ' and ρ'' the perpendicular distances of the observer from the Earth's axis and from the plane of the equator, the unit of these distances being the equatoreal semidiameter of the Earth. The selenocentric spherical triangle between the pole of the lunar equator, the pole of the celestial equator and the observer furnishes the equations:

$$\cos \mathrm{B} \sin \mathrm{P} = -\sin \mathrm{J} \cos (\alpha' - \mathrm{N}),$$
$$\cos \mathrm{B} \cos \mathrm{P} = +\sin \mathrm{J} \sin (\alpha' - \mathrm{N}) \sin \delta' + \cos \mathrm{J} \cos \delta',$$
$$\sin \mathrm{B} = +\sin \mathrm{J} \sin (\alpha' - \mathrm{N}) \cos \delta' - \cos \mathrm{J} \sin \delta',$$
$$\cos \mathrm{B} \sin (\Lambda + \alpha_0 - \mathrm{N}) = \cos \mathrm{J} \sin (\alpha' - \mathrm{N}) \cos \delta' + \sin \mathrm{J} \sin \delta',$$
$$\cos \mathrm{B} \cos (\Lambda + \alpha_0 - \mathrm{N}) = \cos (\alpha' - \mathrm{N}) \cos \delta';$$

from which latter two may be deduced

$$\cos \mathrm{B} \sin (\Lambda + \alpha_0 - \alpha') = [\sin \delta' - \tan \tfrac{1}{2} \mathrm{J} \sin (\alpha' - \mathrm{N}) \cos \delta'] \sin \mathrm{J} \cos (\alpha' - \mathrm{N})$$
$$= (\sin \delta' - \sin \mathrm{B}) \tan \tfrac{1}{2} \mathrm{J} \cos (\alpha' - \mathrm{N}).$$

Hence are derived the formulæ for the computation of P, B and Λ:—

$$\tan \zeta = \tan \mathrm{J} \sin (\alpha' - \mathrm{N}),$$
$$\tan \mathrm{P} = -\tan \mathrm{J} \cos (\alpha' - \mathrm{N}) \cdot \frac{\cos \zeta}{\cos (\zeta - \delta')},$$
$$\tan \mathrm{B} = \tan (\zeta - \delta') \cdot \cos \mathrm{P},$$
$$\sin (\Lambda + \alpha_0 - \alpha') = \frac{\sin \mathrm{B} - \sin \delta'}{2 \cos^2 \tfrac{1}{2} \mathrm{J}} \cdot \sin \mathrm{P},$$
$$\text{or} = \frac{\sin \tfrac{1}{2} (\mathrm{B} - \delta') \cos \tfrac{1}{2} (\mathrm{B} + \delta')}{\cos^2 \tfrac{1}{2} \mathrm{J}} \cdot \sin \mathrm{P},$$
$$\text{or} = \frac{\sin (\zeta' - \delta')}{\cos \zeta'} \cdot \sin \mathrm{P}, \quad \text{if } \tan \zeta' = \tan \tfrac{1}{2} \mathrm{J} \sin (\alpha' - \mathrm{N}).$$

Then follows the computation of the apparent position of the spot. If λ and β are the selenographical longitude and latitude of the spot, and p its position-angle at the centre of the disk, as seen by the observer, we have selenocentrically, if the angular distance between spot and observer is called S,

$$\begin{aligned}
\sin S \sin (p-P) &= \cos\beta \sin(\Lambda-\lambda),\\
\sin S \cos (p-P) &= -\cos\beta\cos(\Lambda-\lambda)\sin B + \sin\beta\cos B,\\
\cos S &= \cos\beta\cos(\Lambda-\lambda)\cos B + \sin\beta\sin B.
\end{aligned}$$

If the linear distance of the spot from the Moon's centre of gravity is k' and from the observer r'', the unit of distances being, as before, the semi-diameter of the Earth's equator, the angular distance s of the spot from the centre of the disk, as seen by the observer, and r'' are found by

$$\begin{aligned}
r'' \sin s &= k' \sin S,\\
r'' \cos s &= r' - k' \cos S;
\end{aligned}$$

or, indirectly, by

$$\sin s = \frac{k'}{r'}\sin(S+s),$$

$$r'' = r' \cdot \frac{\sin S}{\sin(S+s)}.$$

The coordinates

$$\begin{aligned}
x'' &= \omega'' \sin s \sin(p-P), \qquad (\omega'' = \frac{1}{\text{arc } 1''} = 206264''{\cdot}8),\\
y'' &= \omega'' \sin s \cos(p-P),
\end{aligned}$$

or the apparent rectangular coordinates of the spot in reference to the centre of the disk and to the computed direction of the Moon's axis seem to me the most suitable for the determination of the libration. The corrections of x'' and y'' which best satisfy the observations, compared with the variations of x'' and y'' which represent the effect of the corrections of the assumed values of I, λ, β, and of the different terms of the physical libration, furnish then the equations of condition for the determination of the latter.

In the differentiation of the equation for x'' and y''

$$x'' = \frac{\omega'' k'}{r''}\cdot \sin S \sin(p-P) = \frac{\omega'' k'}{r''}\cdot \cos\beta \sin(\Lambda-\lambda),$$

$$y'' = \frac{\omega'' k'}{r''}\cdot \sin S \cos(p-P) = \frac{\omega'' k'}{r''}[-\cos\beta\cos(\Lambda-\lambda)\sin B + \sin\beta\cos B],$$

it must be borne in mind that P as a function of the assumed

elements is itself variable, so that if the corrected values $x''+\delta x''$ and $y''+\delta y''$ are to refer to the same direction P as x'' and y'' themselves, the variation of P must be duly taken into account. Differentiating accordingly, we get

$$\begin{aligned}\delta x'' = \frac{k'}{r''}\Big\{ & \cos\beta\cos(\Lambda-\lambda)\,\delta(\Lambda-\lambda)\\ & -\sin\beta\sin(\Lambda-\lambda)\,\delta\beta\\ & +\sin S\cos(p-P)\,\delta P\Big\}\\ & +x''\cdot\frac{\delta k'}{k'},\end{aligned}$$

$$\begin{aligned}\delta y'' = \frac{k'}{r''}\Big\{ & \cos\beta\sin B\sin(\Lambda-\lambda)\,\delta(\Lambda-\lambda)\\ & +[\cos\beta\cos B+\sin\beta\sin B\cos(\Lambda-\lambda)]\,\delta\beta\\ & -[\cos\beta\cos B\cos(\Lambda-\lambda)+\sin\beta\sin B]\,\delta B\\ & -\sin S\sin(p-P)\,\delta P\Big\}\\ & +y''\cdot\frac{\delta k'}{k'}.\end{aligned}$$

From the expressions of Λ, B, P are derived

$$\begin{aligned}\delta\Lambda = & -\delta a_0-[\tan B\sin(a_0-N+\Lambda)-\tan\tfrac{1}{2}J]\sin J.\delta N\\ & -\tan B\cos(a_0-N+\Lambda)\,\delta J,\\ \delta B = & -\cos(a_0-N+\Lambda)\sin J.\delta N+\sin(a_0-N+\Lambda)\,\delta J,\\ \cos B.\delta P = & -\sin(a_0-N+\Lambda)\sin J.\delta N-\cos(a_0-N+\Lambda)\,\delta J.\end{aligned}$$

But, if

$$M = a_0-N-(l_0-\Omega),$$

then

$$\begin{aligned}\delta a_0 = & \ \delta l_0-(\tan\tfrac{1}{2}J\cos M+\tan\tfrac{1}{2}I)\sin I.\delta\Omega\\ & -\tan\tfrac{1}{2}J\sin M.\delta I,\\ \sin J.\delta N = & -\cos M\sin I.\delta\Omega-\sin M.\delta I,\\ \delta J = & +\sin M\sin I.\delta\Omega-\cos M.\delta I.\end{aligned}$$

Hence

$$\begin{aligned}\delta\Lambda = & -\delta l_0+[\tan B\sin(l_0-\Omega+\Lambda)+\tan\tfrac{1}{2}I]\sin I.\delta\Omega\\ & +\tan B\cos(l_0-\Omega+\Lambda)\,\delta I,\\ \delta B = & \cos(l_0-\Omega+\Lambda)\sin I.\delta\Omega-\sin(l_0-\Omega+\Lambda)\,\delta I,\\ \cos B.\delta P = & \sin(l_0-\Omega+\Lambda)\sin I.\delta\Omega+\cos(l_0-\Omega+\Lambda)\,\delta I,\end{aligned}$$

the substitution of which gives

$$\delta x'' = \frac{k' \cos\beta}{r''}\Big\{ -\cos(\Lambda-\lambda)(\delta l_0 + \delta\lambda) - \tan\beta \sin(\Lambda-\lambda)\,\delta\beta + [\tan\beta \sin(l_0 - \Omega + \Lambda) + \tan\tfrac{1}{2} I \cos(\Lambda-\lambda)] \sin I.\delta\Omega + \tan\beta \cos(l_0 - \Omega + \Lambda)\,\delta I\Big\} + x''\cdot\frac{\delta k'}{k'},$$

$$\delta y'' = \frac{k' \cos\beta}{r''}\cos B\Big\{ -\tan B \sin(\Lambda-\lambda)(\delta l_0 + \delta\lambda) + [1 + \tan\beta \tan B \cos(\Lambda-\lambda)]\,\delta\beta - [\cos(l_0 - \Omega + \lambda) + \tan\beta \tan B \cos(l_0 - \Omega + \Lambda) - \tan\tfrac{1}{2} I \tan B \sin(\Lambda-\lambda)] \sin I.\delta\Omega + [\sin(l_0 - \Omega + \lambda) + \tan\beta \tan B \sin(l_0 - \Omega + \Lambda)]\,\delta I\Big\} + y''\cdot\frac{\delta k'}{k'};$$

or, putting

$$\tan\beta \tan B \sin(\Lambda-\lambda) = \nu \sin(\lambda_1 - \lambda)$$
$$1 + \tan\beta \tan B \cos(\Lambda-\lambda) = \nu \cos(\lambda_1 - \lambda),$$

then

$$\delta y'' = \frac{k' \cos\beta}{r''}\cos B\Big\{ -\tan B \sin(\Lambda-\lambda)(\delta l_0 + \delta\lambda) + \nu \cos(\lambda_1 - \lambda)\,\delta\beta - [\nu \cos(l_0 - \Omega + \lambda_1) - \tan\tfrac{1}{2} I \tan B \sin(\Lambda-\lambda)] \sin I.\delta\Omega + \nu \sin(l_0 - \Omega + \lambda_1)\,\delta I\Big\} + y''\cdot\frac{\delta k'}{k'}.$$

The physical libration produces oscillations in the values of l_0, Ω and I, the theoretical investigation of which is to be found in the *Méc. Cél.* L. v., and in the papers of Poisson (*Conn. des Temps*, for 1821 and 1822), Bessel (*Astr. Nachr.* Nos. 376 and 377, or *Abhandlungen*, iii. pp. 317–328), Wichmann (*Astr. Nachr.* Nos. 619, 621, 628, 630, 631), and Hartwig (*Beitrag zur Bestimmung der physischen Libration des Mondes*, 1880). If i denotes the inclination of the Moon's orbit to the ecliptic, e its eccentricity, ω the angle in the orbit from the node to the perigee, g the Moon's mean anomaly and Σ (H sin h) the sum of the several inequalities of the motion in longitude, which produce sensible terms; if, further, m', n', π' denote the daily motions of the mean longitude, of the node and of the perigee, and h' the daily motion of h, all of which quantities are known: the oscillations of l_0, Ω

and I, due to the physical libration, may be represented by the following equations:—

$$\delta l_0 = \sum\left(-\mathrm{H}\,.\,\frac{\theta\,(1-f)}{1+f\theta}\,.\,\sin h\right) + a_0 \sin\left(\mathrm{A}_0 + \nu_0 t\right),$$

$$\begin{aligned}
\sin \mathrm{I}\delta\Omega = {} & \sin \mathrm{I}\,\delta l_0 \\
& - \kappa q \cos\omega \sin\left(l_0 - \Omega\right) \\
& + \kappa q' f \sin\omega \cos\left(l_0 - \Omega\right) \\
& + b_0 \sin\left[\mathrm{B}_0 + \mu t - \left(l_0 - \Omega\right)\right] \\
& + c_0 \sin\left(\mathrm{C}_0 + \mu' t\right) \cos\left(l_0 - \Omega\right) \\
& + \frac{2c_0}{\sqrt{f}} \cos\left(\mathrm{C}_0 + \mu' t\right) \sin\left(l_0 - \Omega\right),
\end{aligned}$$

$$\begin{aligned}
\delta \mathrm{I} = {} & - \kappa q \cos\omega \cos\left(l_0 - \Omega\right) \\
& - \kappa q' f \sin\omega \sin\left(l_0 - \Omega\right) \\
& - b_0 \cos\left[\mathrm{B}_0 + \mu t - \left(l_0 - \Omega\right)\right] \\
& - c_0 \sin\left(\mathrm{C}_0 + \mu' t\right) \sin\left(l_0 - \Omega\right) \\
& + \frac{2c_0}{\sqrt{f}} \cos\left(\mathrm{C}_0 + \mu' t\right) \cos\left(l_0 - \Omega\right),
\end{aligned}$$

in which f, a_0, A_0, b_0, B_0, c_0, C_0 are the seven constants of the problem which are to be determined, and in which θ, κq, $\kappa q'$, ν_0, μ, μ' have the following values:—

$$\left.\begin{aligned}
\theta' &= \frac{-2n'm'}{h'h'} \cdot \frac{\mathrm{I}}{i+\mathrm{I}} \\
\theta &= \frac{\theta'}{\mathrm{I}-\theta'}
\end{aligned}\right\} \text{for each of the effective inequalities,}$$

$$\kappa q = \frac{-n'e\mathrm{I}}{n'-n'} \cdot \left(\mathrm{I} + \frac{0{\cdot}0391\,i}{i+\mathrm{I}}\right),$$

$$\kappa q' = \frac{-2n'e\mathrm{I}}{n'-n'},$$

$$\nu_0 = \sqrt{-2n'm'} \cdot \frac{\mathrm{I}}{i+\mathrm{I}} \cdot \sqrt{\mathrm{I}-f},$$

$$\mu = m' - \frac{n'\mathrm{I}}{i+\mathrm{I}},$$

$$\mu' = -\frac{4}{3} \cdot \frac{n'\mathrm{I}}{i+\mathrm{I}} \cdot \sqrt{f}.$$

In order to compute the coefficients of the unknown quantities correctly, the values of I and f employed must be sufficiently near the true ones, or the computation will have to be repeated with corrected values. Instead of f itself, it will be preferable to make the correction δf of the best available value of f, the unknown quantity, or rather δf multiplied by a properly chosen factor κ'', so that in the equation for δl_0 the term

$$\sum\left(\frac{\mathrm{H}}{\kappa''} \cdot \frac{\theta\,(1+\theta)}{(1+\theta f)^2} \sin h\right)\kappa''\delta f$$

is to be added to the term

$$\Sigma\left(-\mathrm{H}\,.\,\frac{\theta\,(1-f)}{1+\theta f}\,\sin h\right),$$

which is to be considered as known.

The substitution of the values of δl_0, $\delta\Omega$, and $\delta\mathrm{I}$, which represent the effect of the physical libration, in the previously given equations for $\delta x''$ and $\delta y''$, furnishes then the equations:

$$\begin{aligned}
\delta x'' = &-\frac{\kappa k'\cos\beta}{r''}\times\\
&\Big\{\Big(\cos(\Lambda-\lambda)-\tan\beta\,\tan \mathrm{I}\sin(l_0-\Omega+\Lambda)\Big)\ \Sigma\Big(-\frac{\mathrm{H}\cos \mathrm{I}}{\kappa}\,.\,\frac{\theta\,(1-f)}{1+\theta f}\sin h\Big)\\
&\qquad +q\tan\tfrac{1}{2}\,\mathrm{I}\sin g\cos(\Lambda-\lambda)\\
&\qquad +q\tan\beta\cos(\omega+\Lambda)\\
&\qquad +(q-q'f)\tan\beta\sin\omega\sin\Lambda\Big\}\\
&-\frac{\kappa k'\cos\beta}{r''}\Big\{+\cos(\Lambda-\lambda)\,.\,\frac{\delta\lambda}{\kappa}\\
&\qquad +\tan\beta\sin(\Lambda-\lambda)\,.\,\frac{\delta\beta}{\kappa}\\
&\qquad -\tan\beta\cos(l_0-\Omega+\Lambda)\,.\,\frac{\delta \mathrm{I}}{\kappa}\\
&+\Big[\ [\cos(\Lambda-\lambda)-\tan\beta\tan \mathrm{I}\sin(l_0-\Omega+\Lambda)]\ \Sigma\Big(\frac{\mathrm{H}\cos \mathrm{I}}{\kappa\kappa''}\,.\,\frac{\theta\,(1+\theta)}{(1+\theta f)^2}\sin h\Big)\\
&\qquad -\frac{q'}{\kappa''}\tan\beta\sin\omega\sin\Lambda\Big]\kappa''\delta f\\
&+\Big(\cos(\Lambda-\lambda)-\tan\beta\tan \mathrm{I}\sin(l_0-\Omega+\Lambda)\Big)\,.\,\frac{a_0}{\kappa}\sin(\mathrm{A}_0+\nu_0 t)\\
&\qquad +\tan\beta\,.\,\frac{b_0}{\kappa}\cos(\mathrm{B}_0+\mu t+\Lambda)\\
&\qquad -\tan\beta\,.\,\frac{2}{\sqrt{f}}\,.\,\frac{\cos\Lambda}{\cos\Lambda_1}\,.\,\frac{c_0}{\kappa}\cos(\mathrm{C}_0+\mu' t-\Lambda_1)\Big\},\\
&\qquad\qquad \text{if } \tan\Lambda_1=\frac{\sqrt{f}}{2}\tan\Lambda\\
&+\frac{x''}{100}\,.\,\frac{100\,\delta k'}{k'};
\end{aligned}$$

and, putting as before

$$\begin{aligned}
\nu\sin(\lambda_1-\lambda) &= +\tan\beta\,\tan \mathrm{B}\sin(\Lambda-\lambda),\\
\nu\cos(\lambda_1-\lambda) &= 1+\tan\beta\,\tan \mathrm{B}\cos(\Lambda-\lambda),
\end{aligned}$$

then

$$\delta y'' = -\frac{\kappa k' \cos\beta}{r''} \cdot \cos B \cdot \nu \times$$

$$\left\{\left(\tan I \cos(l_0 - \Omega + \Lambda) + \frac{\tan B}{\nu}\sin(\Lambda-\lambda)\right)\ \Sigma\left(-\frac{H\cos I}{\kappa}\cdot\frac{\theta(1-f)}{1+\theta f}\sin k\right)\right.$$

$$+ q \tan\tfrac{1}{2} I \sin g \cdot \frac{\tan B}{\nu}\sin(\Lambda-\lambda)$$

$$+ q \sin(\omega+\lambda_1)$$

$$\left.+ (q-q'f)\sin\omega\cos\lambda_1\right\}$$

$$-\frac{\kappa k'\cos\beta}{r''}\cdot\cos B\cdot\nu\left\{+\ \frac{\tan B}{\nu}\sin(\Lambda-\lambda)\frac{\delta\lambda}{\kappa}\right.$$

$$+\ \cos(\lambda_1-\lambda)\frac{\delta B}{\kappa}$$

$$\left.-\ \sin(l_0-\Omega+\lambda_1)\cdot\frac{\delta I}{\kappa}\right.$$

$$+\left[\left(\tan I\cos(l_0-\Omega+\Lambda)+\frac{\tan B}{\nu}\sin(\Lambda-\lambda)\right)\ \Sigma\left(\frac{H\cos I}{\kappa\kappa''}\cdot\frac{\theta(1+\theta)}{(1+\theta f)^2}\sin k\right)\right.$$

$$\left.+\frac{q'}{\kappa''}\sin\omega\cos\lambda_1\right]\kappa''\delta f$$

$$+\left(\tan I\cos(l_0-\Omega+\Lambda)+\frac{\tan B}{\nu}\sin(\Lambda-\lambda)\right)\cdot\frac{a_0}{\kappa}\sin(A_0+\nu_0 t)$$

$$+\ \frac{b_0}{\kappa}\sin(B_0+\mu t+\lambda_1)$$

$$\left.+\ \frac{\cos\lambda_1}{\cos\lambda_{11}}\cdot\frac{c_0}{\kappa}\sin(C_0+\mu' t-\lambda_{11})\right\},$$

$$\text{if } \tan\lambda_{11} = \frac{2}{\sqrt{f}}\tan\lambda,$$

$$+\frac{y''}{100}\cdot\frac{100\,\delta k'}{k'}.$$

The factor κ, which is introduced on account of obvious convenience, is arbitrary, and may be put $=240$ or, perhaps better, $=300$. The most convenient value of κ'', the factor of δf, will be that of the largest of the coefficients

$$\frac{H}{\kappa}\cdot\frac{\theta(1+\theta)}{(1+f\theta)^2},$$

or that dependent on the Moon's annual equation.

I have written out the equations for $\delta x''$ and $\delta y''$ in full, retaining all the terms in symbolical form, so as to leave the question of the best numerical values which ought to be employed an open one. The substitution of numerical terms and the introduction of suitable abbreviations will, of course, greatly simplify the equations, especially in case the selected spot is near the lunar equator, a case the advantages of which are apparent at a glance.

The measures from which the observed values of $\delta x''$ and

$\delta y''$ are to be deduced furnish a series of angular distances $s_1 \ldots s_n$ of the spot from points of the Moon's limb in various position-angles $p_1 \ldots p_n$, the corresponding times being $t_1 \ldots t_n$. For these times the values of

$$\alpha', \delta', r', R', P, B, \Lambda, x'', y''$$

(R′ being the Moon's apparent semi-diameter, $\sin R' = \frac{k}{r'}$), may be found by interpolation from the values of these quantities computed at intervals of, say, 20 or 30 minutes for the extent of each set of measurements. From the distances and position-angles, duly cleared from instrumental errors and from the effects of refraction, the corresponding semi-diameters $R_1 \ldots R_n$ are to be derived by means of the equations

$$\begin{aligned}
s_1 \sin (p_1 - P_1) + x_1'' &= R \sin Q_1, \\
s_1 \cos (p_1 - P_1) + y_1'' &= R_1 \cos Q_1, \\
&\ldots\ldots \\
s_n \sin (p_n - P_n) + x_n'' &= R_n \sin Q_n, \\
s_n \cos (p_n - P_n) + y_n'' &= R_n \cos Q_n,
\end{aligned}$$

in which $Q_1 \ldots Q_n$ are the position-angles of the observed points of the limb at the centre of the disk, reckoned from the direction of the Moon's axis.

The comparison of these $R_1 \ldots R_n$ derived from observation, with the interpolated values $R'_1 \ldots R'_n$ supplies the equations of condition

$$R_1' - R_1 = \sin Q_1 \, \delta x'' + \cos Q_1 \, \delta y'' - \frac{R'}{R_0} \delta R_0,$$

$$\ldots\ldots$$

$$R'_n - R_n = \sin Q_n \, \delta x'' + \cos Q_n \, \delta y'' - \frac{R'}{R_0} \delta R_0,$$

from which the corrections $\delta x''$, $\delta y''$ and δR_0 for each set of measures are to be deduced. The correction δR_0 is that of some conveniently chosen mean value R_0, and, in case the tabular parallaxes can be depended on, is common to all the sets of measures made with the same instrument under similar circumstances.

The substitution of the corrections $\delta x''$ and $\delta y''$ derived from observation in the previously given equations leads to the equations of condition for the determination of $\delta\lambda$, $\delta\beta$, δI, δf, and the others terms of the physical libration.

In order to be able to trace the influence of the irregularities of the Moon's limb, it will be well to compute the selenographical positions of the several points of the limb, which have been measured in position-angle and distance in reference

to the spot. Their selenographical longitudes λ_n and latitudes β_n are derived from the corresponding values of Q_n, Λ_n, B_n by means of the equations

$$\cos\beta_n \sin(\lambda_n - \Lambda_n) = -\sin Q_n,$$
$$\cos\beta_n \cos(\lambda_n - \Lambda_n) = -\cos Q_n \sin B_n + \tan R_n \cos B_n,$$
$$\sin\beta_n = +\cos Q_n \cos B_n + \tan R_n \sin B_n,$$

which in practice, where great accuracy is not required, may be sufficiently simplified.

Although I have left the question of the best numerical values which ought to be substituted in the formulæ an open one, some remarks which bear upon it and touch a few of the practical difficulties of the investigation will not be out of place.

An uncertainty in the determination of I is occasioned by the difficulty of disentangling δI and b_0, in case the observations do not extend over a sufficiently long period. The angle $B_0 + \mu t$ is

$$= B_0 + m't - \frac{n'I}{i+I}t,$$
$$= B_0 - (l_0 - \Omega)_0 + l_0 - \Omega + \frac{n'i}{i+I} \cdot t,$$

if $(l_0 - \Omega)_0$ is the value of $l_0 - \Omega$ for the time $t=0$, from which t is reckoned; or, putting

$$B_0 - (i_0 - \Omega)_0 = B_1, \text{ and}$$
$$\frac{n'i}{i+I} = \mu_1,$$
$$B_1 + \mu t = B_1 + l_0 - \Omega + \mu_1 t.$$

Hence in the equation for $\delta y''$

$$b_0 \sin(B_0 + \mu t + \lambda_1) = (b_0 \sin B_1)\cos(l_0 - \Omega + \lambda_1 + \mu_1 t) + (b_0 \cos B_1)\sin(l_0 - \Omega + \lambda_1 + \mu_1 t).$$

But the coefficient of $-\delta I$ is $\sin(l_0 - \Omega + \lambda_1)$, so that the two angles differ only by $\mu_1 t$. As the period of this difference is about twenty-four years, it is obvious that observations extending only over a year or two do not allow the satisfactory determination of both unknown quantities δI and $b_0 \cos B_1$, and that one of the two must remain practically indeterminate.

A troublesome difficulty arises from the necessity of repeating a portion of the computations, in case the adopted value f is not sufficiently near the true one. Wichmann's first value $f = 0{\cdot}2603$ was derived from a preliminary solution of his longitude-equations, and it served, together with Nicollet's value of

$I = 1° 28' 47''$, in the further computations. Wichmann's final deductions gave

$$I = 1° 32' 9''; \quad f = \frac{789''\cdot7}{1884''} = 0\cdot419.$$

or, neglecting a_0 and b_0 on account of the uncertainty of their determination,

$$I = 1° 32' 24''; \quad f = \frac{838''\cdot6}{1884''} = 0\cdot445.$$

It is to be regretted that Hartwig, instead of adopting one of these latter values, has retained in his computations the preliminary inaccurate approximations. His own final result (*Monthly Notices* for May, p. 376),

$$I = 1° 36' 39''; \quad f = 0\,507,$$

gives for ν_0 the value $1458''\cdot5$, or the period 888·6 days. Wichmann's (last) result gives $1520''\cdot9$, or period 852·1 days, while the preliminary value employed is $1728''\cdot9$, or the period 749·6 days. How far the determination of a_0 is affected by the error of the assumed value of ν_0 is at present unknown; but it need scarcely be more than hinted that the error affects most seriously the connection between the results of the observations of 1845 and of 1878. Bessel points out in Article 6 of his paper that, in case a sensible value of a_0 should be successfully ascertained, the determination of the duration of its period would afford the surest means for the exact determination of f. According to the data on page 376 the value of a_0 is found from

$$\text{Wichmann's Observations} = 183''\cdot2 \pm 80''6$$
$$\text{Hartwig's Observations} = 62\cdot3 \pm 41\cdot9.$$

It is to be hoped that before long the publication of the investigation of the Moon's libration which has been undertaken at the Oxford University Observatory will facilitate some decision of what are the best available values of I and f.

In a second paper I shall treat the problem of determining the physical libration by observations of the changes which it produces in the relative position of spots; I shall, further, show the effect of the libration upon the observations of a spot in right ascension and declination, and also consider some other questions.

Apparent Places of Comet b 1881 *from Observations made with the North Equatoreal of the Melbourne Observatory. Aperture* 4·5 *inches.* By R. L. J. Ellery, Esq., F.R.S.

Melbourne Mean Time.	Observer.	Number of Measures.	Comet−Star. R.A.	Comet−Star. N.P.D.	Comet. R.A.	Comet. N.P.D.	Star.
d h m s			m s	′ ″	h m s	° ′ ″	
May 23 6 17 17·3	W.	4	+ 2 16·72	+ 2 54·22	4 58 48·61	125 13 32·3	Lacaille 1685
24 6 22 28·9		3	− 5 39·57	+ 4 28·73			*a*
24 6 33 18·3		2	− 8 58·94	+ 7 25·11			*b*
25 18 18 56·2		2	− 4 16·12	+ 2 38·74			*c*
26 6 10 1·6		3	− 5 5·41	+ 0 35·42			*d*
26 6 17 19·5		4	−11 45·12	+ 0 33·70	5 0 16·62	123 40 44·9	B.A.C. 1644
28 18 10 56·9		3	+ 3 42·93	+ 7 4·91	5 1 35·63	122 3 49·0	B.A.C. 1564
29 5 39 5·0		6	+ 3 55·81	−14 2·46	5 1 48·51	121 42 41·4	B.A.C. 1564
31 18 22 37·5		5	+ 7 54·15	−11 5·73	5 3 12·43	119 34 13·3	Washington 2127
June 1 5 25 19·2		9	+ 0 27·56	− 1 23·01	5 3 26·26	119 6 40·0	„ 2173
3 5 27 4·4		1	−13 48·78	+ 4 25·75	5 4 36·52	116 53 37·1	„ 2251
3 5 27 4·4		1	+ 1 23·70	− 3 35·23			*e*
3 6 4 21·1		4	+ 3 16·28	+12 37·37	5 4 37·65	116 51 36·9	Washington 2163

Melbourne Mean Time.	Observer.	Number of Measures.	Comet—Star. R.A.	Comet—Star. N.P.D.	Comet. R.A.	Comet. N.P.D.	Star.
d h m s			m s	′ ″	h m s	° ′ ″	
June 4 18 15 58·8	W.	5	−11 17·45	− 2 13·56	5 5 36·14	114 51 10·3	Washington 2240
5 18 0 43·7		7	+ 1 49·10	+ 4 55·35			*f*
6 18 17 23·6		1	− 2 4·73	− 8 39·28	5 6 56·21	111 38 44·3	Lalande 9853
8 5 36 37·1		1	− 0 12·38	+ 5 41·66			*g*
8 18 17 42·1		5	+ 0 26·64	− 4 58·90			*h*
8 18 30 4·5		1	−19 1·39	−16 31·79	5 8 28·06	107 38 1·7	α Leporis
9 5 17 58·5		7	+ 1 11·37	+15 38·96	5 8 47·23	106 36 27·9	μ „
10 5 25 43·0		5	− 8 24·90	+ 4 59·94	5 9 39·29	104 7 25·2	8 „
10 6 3 13·6		4	+ 0 32·43	− 0 0·60			*i*
12 17 52 57·9		4	+ 0 20·66	−22 33·10	5 12 11·39	96 35 54·8	τ Orionis
12 18 30 45·3		6	− 0 33·93	+ 8 34·76			*k*

tude. The tail had also increased in length and visibility, and could be traced to about 12° or 13° from the nucleus. It extended to 2° or 3° beyond *o Columbæ*, which star it grazed with its upper edge. When viewed through the 3-in. refractor the coma seemed much broader than on the previous evening, and was fully 6′ in diameter. The width of the tail had increased to 12′ to 15′ at its widest part. The fact that this comet is increasing in brightness, tends to show, I think, that it is yet approaching us, and its slow change of apparent position with reference to the star in its vicinity would show that it is at present describing the arc of a very large orbit as it nears the Sun from the depths of space. But I fear it is yet too soon to speculate, for the very unfavourable state of our heavens which has prevailed ever since the first appearance of this comet, has quite prevented any reliable observation even with the naked eye, so that, indeed, our visitor may possibly be diminishing in brightness rather than increasing.

Wednesday, June 1st.—Evening beautifully clear, not a cloud in the sky. Comet visible to the naked eye at 5·30 P.M.; young moon very bright. Comet situated 5 hours 8 min. R.A. 28° S.D. approximately. The 9½-inch Calver revealed a beautiful object, somewhat resembling Pape's drawing of Donati's comet of 1858, and Bro ·ie's of Coggia's comet of 1874. The nucleus appeared to be inclosed in a cometic envelope or series of envelopes of parabolic form. It was not, however, situated in the focus of this curve, as was the case with the comets mentioned above, but was on one side of this point, by about the breadth of its own diameter on the side towards the Sun. The immediate focus was occupied by a condensed stream of coma, issuing, as it were, from the nucleus. This stream was bounded on its exterior edge by a darker interval, also partaking of the parabolic form, but not extending over the nucleus. There was another dark rift of similar form around the cometic envelope near the interior limits of the comet; beyond this a faint light fading away to invisibility. There was also a dark interval immediately behind the nucleus, which gave it a sharp definition on its exterior edge. The tail appeared like the continuation of this luminous parabolic envelope, widening out as its distance from the focus increased, and fading away to invisibility as the luminous matter of which it is composed became more and more rare. The tail seemed to the naked eye much shorter than last evening, but this may be owing to the increased moonlight. It terminated near *o Columbæ*. The movement of this body is N.W., so that it will set earlier each evening. It set on this occasion at 7·40 P.M.

Thursday, June 2.—Sky clear till 7 P.M. when it again clouded over. Saw comet about $5^h\ 45^m$ P.M. It had moved a little to the N.W. of its position last evening, and was now about $5^h\ 10^m$ R.A. and 27° S.D. Tail could still be traced with naked eye to *o Columbæ*. When viewed through the 9½-inch Calver it was

immediately apparent that the comet had undergone a vast change since I had observed it on the previous evening, and presented quite a new character. The nucleus was still out of the focus of the parabola, but the dense stream of cometic matter which appeared to issue from it laterally on the evening before had entirely disappeared, as had also the dark interval surrounding it; but streams of light seemed now to issue in a spiral form from the exterior edge of the nucleus, and to be beaten back again towards the tail after they had reached the outer limits of the coma, and were beaten back on that side that was farthest from the nucleus. There was still a dark interval on the interior edge of the nucleus. A great portion of the detail which existed on the previous evening had disappeared, and these spiral streams issuing from the nucleus constituted the principal feature on this occasion. The nucleus appeared slightly larger but less condensed; the gradation of light from nucleus to coma was less sudden, and the coma above the nucleus somewhat brighter. The increased moonlight interfered greatly with observation.

Friday, June 3.—Cloud cleared off at sunset. Comet visible to naked eye at $6^h\ 40^m$ P.M. Tail could still be traced to *o Columbæ*, which star it grazed with its lower edge. When viewed through the $9\frac{1}{2}$-inch Calver, there was no great apparent change from last evening. The detail, however, was still more diminished by the increased moonlight; the nucleus seemed nearer to the centre of the parabola, the spiral streams emanating from the nucleus less marked, and the width of the coma narrower.

Saturday, June 4.—Evening fine. Comet visible $5^h\ 40^m$. Situated a little east of a straight line extending from *α Columbæ* to *ε Leporis*, and about as far from the latter as *α Columbæ* is from *β Columbæ*. Tail very long. When viewed with the reflector the spiral character of the jet from the nucleus could no longer be detected. Moonlight strong.

Sunday, June 5.—Early part of evening favourable. Comet visible at $6^h\ 35^m$ P.M. Tail very long (about 14°) and very straight. Through the $9\frac{1}{2}$-inch, owing to the bright background, very little visible except the nucleus and upward jet of cometary vapour slightly inclined, and bent over towards the opposite side from where the nucleus was situated.

Monday, June 6.—Cloudy.

Tuesday, June 7.—Saw comet at $5^h\ 20^m$ A.M., not long risen. Tail appeared shorter but broader than in the evening light. Too low in horizon for observation, and when it rose higher it was soon eclipsed by the approaching sunlight. Evening, cloudy.

Wednesday, June 8.—Saw comet for a few moments only at $5^h\ 45^m$ P.M. Very bright, but low in the horizon. Wet, cloudy.

Thursday, June 9.—More clouds. Comet visible for a short time about 6 P.M. Situated about 45′ north of *μ Leporis*.

Graham's Town, Cape of Good Hope,
June, 1881.

Observations of Comet b 1881 *made at Monte Video.*
By Lieut. B. E. W. Gwynne, R.A.

The diagram sent represents the approximate position of a comet with reference to the surrounding stars, seen from H.M.S. *Garnet* whilst laying at Sandy Point Road (Straits of Magellan) on the night of May 30, 1881, at 6.30 P.M. Ship Mean Time. I only had time to take two good sextant angles with *Sirius* and *Canopus*, which were: corrected 26° 18′ (*Canopus*), 26° 10′ (*Sirius*); approximate R.A. of comet $5^h\ 2^m$; Decl. of Comet 26° 40′ S. The later observations were as follows:—

Date 1881.	Sirius.	L.M.T.	Canopus.	L.M.T.	Achernar.	L.M.T.	Remarks.
May 31	25 38 30	6 5	27 14 10	6 10	45 39 10	6 15	Latitude and Longitude of Observer
Bar. 29·80							
Ther. 33							
June 1	25 9 10	5 56	28 7 0	6 0	46 30 50	6 3	53 10 20 S.
Bar. 29·94							
Ther. 36							
June 2	24 34 0	5 45	29 2 50	5 49	47 30 30	5 52	70 53 17 W.
Bar. 30·10							
Ther. 40							

The above are carefully observed sextant angles (corrected for index error) between the comet's nucleus and the stars *Sirius*, *Canopus*, and *Achernar*.

The following will give a good idea of the position of the comet on the evening of June 1, at 6 P.M. Ship Mean Time.

A line drawn from *Sirius* at right angles to a line joining *Canopus* and *Rigel*, and produced 4° beyond it, passed through the nucleus of the comet.

The axis of the comet's tail was parallel to the line joining *Canopus* and *Rigel*.

The nucleus presented the appearance of a planet (such as *Jupiter*) with its brilliancy "dulled" by the nebulous matter that slightly precedes as well as surrounds it and also forms its tail. The latter subtended an arc of about 4°, and pointed towards *Canopus*, whilst the nucleus points to *Rigel*.

Observations of Comet b 1881 *made at Pretoria.*
By E. C. Markwick, Esq.

I send a short account of a comet which has been lately visible in the Southern Hemisphere, and which has been to an astronomical eye a beautiful object.

On the evening of May 30, about 6.30 P.M. local time, my attention was attracted by a nebulous-looking star in the S.W. quarter of the sky, and not 20° above the horizon. In a few moments I could detect a faint tail, and on looking at it through a small achromatic of $1\frac{3}{4}$ inches aperture, saw at once that it

was a very pretty comet, with a brilliant head. No distinct nucleus could then be seen, but the central brilliancy was gradually softened off all round with a woolly light. The tail seemed to start back in a straight line opposite to the Sun's direction, and faded away rapidly, although with the naked eye I should estimate it might be traced some 4° or 5°, if not more. It was inclined to the horizon at an angle of between 30° and 40°. The head was situated at the apex of a right-angled triangle constructed on the line joining *β Columbæ* and *α Leporis*, and, as far as I could judge roughly (on comparison with Proctor's smaller star atlas) was in R.A. 5^h 0^m, S.D. 32°.

On May 31, at 6·30 P.M. the general appearance was unchanged; perhaps it was a shade brighter. Position roughly estimated, R.A. 5^h 0^m, S.D. 30°. Head about equal distances from *Sirius* and *Canopus*. In intrinsic brightness I estimated it between *β* and *γ* Leporis, or between 4th and 5th mag., but owing to there being quite a disk visible to the naked eye, the head appeared more like a 2nd mag. star. With the telescope there was a distinct nucleus visible in the head, although rather dull. The upper edge of the tail passed just over *o Columbæ*, and I could not fairly trace it beyond, although faint glimpses of a further extension could occasionally be caught by averted vision.

June 1.—Comet perceptibly increased in brilliancy. At 6.10 P.M. I took alignments, and found the head as near as might be in a line with *β* and *α Columbæ*, and two-thirds the distance between *β* and *α Leporis*, below a line drawn through those stars towards *Columba*. Hence I estimate its position at R.A. 5^h 0^m, S.D. 28° 30′. The direction of the tail was from this point towards *ζ Pictoris*, the direction having apparently changed since last night. Nucleus woolly, and not well-defined. To the naked eye it looked like a beautiful, dull rocket. A faint star could be seen through its tail, and several near.

June 2.—Cloudy; no satisfactory observations.

June 3.—First seen at 6 P.M. Undoubtedly brighter; but it is getting low in the west, and the increasing moonlight interferes much with its appearance. Taking angles with a pocket sextant (without corrections for refraction), I found the head to be—

At 7 P.M., 24° 0′ from *Sirius*,
and 29° 55′ from *Canopus*.

Taking eye observations, I noted it as three-fourths the distance between *α* and *β Leporis*, below a line joining those stars and produced; and two-thirds the distance between *α* and *β Columbæ* to the right or N. of a line joining those stars and produced.

June 4.—Comet very badly situated for observation. Angles with sextant:—

At 6^h 35^m P.M., 23° 43′ from *Sirius*.
At 6^h 40^m P.M., 30° 53′ from *Canopus*.

June 5.—Comet well seen, considering its bad position for observation. At $6^h\ 5^m$ P.M. noted the nucleus as star-like in the telescope, but later on this was not so apparent. I have some idea a star might have been in close contiguity to the nucleus. Angles :—

At $6^h\ 20^m$ P.M. 23° 4′ from *Sirius*.
32° 16′ „ *Canopus*.

June 6.—Nucleus seen. Brightness of the comet has, I fancy, increased, but it is much obscured by moonlight. Tail could be traced for some 5° or more.

At $6^h\ 15^m$ P.M., 22° 31′ from *Sirius*.
33° 49′ „ *Canopus*.
17° 6′ „ *β Canis*.

June 7.—Comet getting much brighter, although obscured greatly by moonlight. Position :—

At $6^h\ 10^m$ P.M., 22° 10′ from *Sirius*.
At $6^h\ 19^m$ P.M., 35° 28′ „ *Canopus*.

June 8.—Approaching the Sun rapidly. Position :—

At $6^h\ 17^m$ P.M. 21° 53′ from *Sirius*.
37° 25′ „ *Canopus*.

Since the last date I have not seen it, and conclude it has disappeared below the horizon.

I forward two sketches, one showing the general appearance of the comet to the naked eye, the other as seen in a small achromatic of about two inches aperture. There was no marked change in the general features during the whole period of its visibility. These sketches, however, represent it, I think, pretty fairly for June 1 at 6.30 P.M.

Pretoria, Transvaal,
South Africa :
1881, *June* 16.

Elements of Comet b 1881. By A. Graham, Esq.

The calculations were founded on the following meridian observations :

	G.M.T.		
	h m s	h m s	° ′ ″
1881. June 23	11 30 54·4	$\delta\alpha = 5\ 34\ 55{\cdot}2$	$\delta\Delta = 44\ 53\ 20{\cdot}6$ Oxford
29	11 37 39·3	6 5 20·5	24 11 37·9 Oxford
July 4	11 58 36·3	6 51 29·1	14 47 0·9 Cambridge

After correcting for Aberration and Parallax from the elements by Dr. H. Oppenheim, given in the *Dun Echt Circular*, No. 24, a careful calculation gave:

$T = 1881$, June 16·45914 Greenwich Mean Time.

$\pi = 265^\circ\ 14'\ 46''.4$ Mean Equinox 1881·0
$\Omega = 270\ 58\ 1''.2$ „
$i = 63\ 28\ 20''.0$
$\log. q = 9.865812$

Heliocentric motion, direct.

The calculated middle place compared with the observed, gives:

C − O in longitude $+2''.6$ in latitude $+2''.8$

The heliocentric equatoreal coordinates, referred to the apparent equinox, June 30·0, are:

$x = [9.650203] \sin (356^\circ\ 28'\ 10''.6 + v)$ $d \log a = +.144 d\Omega$, $dA = +2.237 d\Omega$
$y = [9.991862] \sin (243\ 26\ 31''.4 + v)$ $d \log b = -.344 d\Omega$, $dB = +0.384 d\Omega$
$z = [9.961381] \sin (328\ 29\ 46''.3 + v)$ $d \log. c = +.362 d\Omega$, $dC = +0.091 d\Omega$

The corrections to the logarithms are to be applied to the units in the sixth decimal place, $d\Omega$ is expressed in seconds of arc, and v represents the true anomaly.

On 1881, July 13, with the Northumberland Equatoreal and square bar micrometer, five careful comparisons were made with B.A.C. 3199, which is contained in the Greenwich Observatory Catalogue of 2263 Stars, No. 908. Using the Greenwich place, the results, corrected for Parallax and Aberration, are:

1881, July 13·451648 G.M.T. $\Delta\alpha = 9^h\ 18^m\ 33^s.83$, $\Delta\delta = 81^\circ\ 49'\ 18''.5$

An Ephemeris calculated from my elements gives:

$\Delta\alpha = 9^h\ 18^m\ 33^s.79$, $\Delta\delta = 81^\circ\ 49'\ 20''.9$

There is thus hardly any room for doubt that the orbit is very nearly a parabola.

The Observatory, Cambridge:
1881, *September* 9.

Observations and Elements of Comet b 1881.

By J. Tebbutt, Esq.

By a previous mail I sent to Europe my report of the appearance of a comet, and I have now the honour to supplement it by my observations and an approximate determination of the orbit. The following positions, with the exception of that for June 11, were obtained with the filar micrometer of the 4½-inch Equatoreal, the differential measures being all corrected for refraction. The corrections for parallax may be obtained by means of the log. factors $\frac{p}{P}$, $\frac{q}{P}$, p and q denoting the corrections in time and arc respectively, and P the comet's equatoreal horizontal parallax in arc. When the comet was first detected no tail could be distinguished without optical aid. Traces of a tail were, however, seen without a telescope on the following evening, and on the 25th it was about two degrees in length. Measures on the evening of June 1 gave 8½ degrees as the length of the appendage, but increasing moonlight prevented its being seen to advantage. The nucleus was brilliant and well defined in the telescope. The comparisons with Lacaille 1685 were not good, owing to the proximity of the objects to the horizon. Those of June 4 were also unsatisfactory, and on the morning of the 12th the comet was only compared in the twilight with *Rigel* by means of the circles of the Equatoreal.

Windsor Mean Time 1881.	App. R.A. of Comet	Log. Factor for Parallax $\frac{p}{P}$	App. N.P.D. of Comet	Log. Factor for Parallax $\frac{q}{P}$	No. of Cp.	Cp. St.
h m s	h m s		° ′ ″			
May 22, 6 56 24	4 58 20·90	+8·8351	125 39 54·9	+9·6475	8	1
23, 7 1 17	4 58 52·38	+8·8326	125 13 45 0	+9 6666	7	2
23, 7 35 26			125 13 2·8	+9·7287	3	3
23, 7 39 19	4 58 52·60	+8·8243			2	3
25, 7 3 37	4 59 48·23	+8·8264	124 13 16·7	+9·6886	7	4
27, 18 9 44	5 1 3·46	−8·8166	122 45 53·7	+9·7163	1	5
June 1, 6 22 4	5 3 27·15	+8·8034	119 5 16·5	+9·6795	12	6
2, 6 42 37	5 4 2·86	+8·7970	118 0 55·7	+9·7195	1	7
4, 6 46 24	5 5 13·65	+8·7851	115 32 55·9	+9·7387	3	8
5, 6 25 42	5 5 53·74	+8·7835	114 7 40·5	+9 7216	5	9
6, 6 14 30	5 6 33·29	+8·7792	112 32 31·3	+9·7149	5	10
6, 6 14 30	5 6 33·22	+8·7792			5	11
8, 6 12 59	5 8 0·91	+8·7680	108 46 24·4	+9·7288	5	12
11, 18 27 36	5 11 8·08	−8·7454	99 47 52·1	+9·7154	3	13

Adopted Mean Places of the Comparison Stars for 1881·0, *and Apparent Places for the Dates of Observation.*

Comp. Star.	R.A. Mean. h m s	App. s	N.P.D. Mean. ° ′ ″	App.	Authority for Star's Mean Place.
1	5 0 7·62	7·50	125 38 49·1	52·9	$\frac{\text{Wash. Gen. Cat. 1860} + \text{Wash. Obs. 1875} + \text{Cape Cat. 1860}}{3}$
2	5 2 8·52	8·42	125 11 42·9	46·6	*Lacaille's Cat.* 1750, No. 1722
3	4 56 34·32	34·22	125 11 5·9	9·0	„ „ No. 1685
4	4 55 40·27	40·20	124 20 16·6	19·0	„ „ No. 1681
5	5 11 58·40	58·42	122 38 39·0	42·5	„ „ No. 1785
6	5 2 58·53	58·70	119 8 1·0	2·3	*Wash. Gen. Cat.* 1860, No. 2173
7	5 32 34·62	34·86	117 56 28 9	33·1	„ „ No. 2342
8	5 11 43·63	43·95	115 27 51·0	52·5	„ „ No. 2217
9	5 8 9·50	9·87	114 6 21·0	21·9	*Wash. Mural Cir. Zones*, 156, 1848, No. 11
10	5 0 25·36	25·80	112 31 56·0	55·7	*Nautical Almanac*, 1881
11	5 4 42·20	42·63			*Ann. de l'Obs. Roy. de Bruxelles Cat.* 1874, No. 116
12	5 8 12·07	12·63	108 41 50·4	50·7	*Oeltzen Argelander, Sud.* No. 3773
13	5 8 49·11	49·97	98 20 26·1	25·9	*Nautical Almanac*, 1881

In bringing up the places of the stars the proper motion has been taken, where possible, from the B.A. Catalogue. It will be necessary to re-determine the places of Lacaille's stars in the meridian at the close of the year.

From the comet's positions for May 22, June 1 and 11, uncorrected for Aberration and Parallax, I have deduced the following parabola:—

Perihelion passage, 1881, June 16·30995, Greenwich Mean Time.

Longitude of the perihelion	...	264° 56′ 16″ M. Eq. 1881·0
Longitude of the ascending node	...	270° 54′ 0″ „
Inclination of the orbit	... ...	63° 27′ 14″
Perihelion distance	... ...	0·7357075
Motion	... ...	Direct.

These elements resemble those of the Great Comet of 1807, and were it not that Bessel assigned the long period of 1714 years to that comet, I should regard the late visitor as identical with it. At the time of the comet's nodal passage the Earth was at no great distance from the prolongation of the axis of the comet's tail, and it will be seen that the comet's orbit approached very near to that of the planet *Venus*.

Observatory, Windsor, N. S. Wales:
1881, *August* 10.

Fall of a Meteorite on March 14, 1881. By Prof. A. S. Herschel, M.A., F.R.A.S.

A stonefall took place at 3.35 p.m. on the above date a mile and three quarters from Middlesborough, in Yorkshire, along the branch line of the North Eastern Railway from Middlesborough to Guisborough, at a place known as Pennyman's Siding on the railway. The fall was accompanied by the usual thunder-like report, not heard at the place where the meteorite struck the earth, but as far off as Northallerton and Welbury in Yorkshire.

Some workmen's attention on the railway was drawn for about four seconds to a whirring noise overhead, followed immediately by a heavy thud in the ground near them; and on searching in the direction indicated by the sound, they found the stone, about three minutes afterwards, at the bottom of a hole eleven or twelve inches deep which had formed almost vertically through an inch of coke ballast and through thin growing turf and stony clay below at the foot of the slight embankment of the railway, four yards from the nearest line of rails, nineteen yards from the signal-box of the siding, and forty-eight yards from the place where they stood when they heard the sound. The foreman narrated the occurrence, and placed the stone in the hands of the engineer of the Darlington district of the railway, Mr. Cudworth, in whose possession it now remains as property of the railway company; but it was submitted to me on March 25 for examination, and on Saturday, March 26, I visited the place of fall with Messrs. Cudworth and Ellinor and the workmen under them, and with some scientific friends. A photograph of the site, and of the group of men finding the stone, has since been made, and steps are being taken for preserving the hole in the ground in a box fitted and screwed together round the earth about it, which will be thus bodily removed.

The stone weighs 3 lbs. 8 oz. 83 grains, and is of a low pyramidal shape like an upper oyster-shell, three inches thick and rather less than 6 in. × 5 in. in length and breadth. The interior is visible at points of the frayed edge and is grey, with very little interspersed grains of iron pyrites, and apparently no iron; and a magnet is not sensibly affected by the mass. Its specific gravity roughly determined is a little greater than 3·0. The flat back surface of the meteorite is covered with a rough brown crust, while the blunt conical front surface is deeply scored and furrowed radially from the centre, and polished like fresh molten slag and of a lead-grey colour.

The singular form and contour of the stone make it very desirable that, whatever provision is finally made for its preservation and mineralogical examination and description, it should not undergo more defacement from its original integrity than is absolutely necessary.

On a simple Approximate Method of calculating the Effect of Refraction upon the Distance and Position-angle of Two Adjacent Stars. By Robert S. Ball, Esq., Royal Astronomer of Ireland.

In connection with the Parallax Observations on which we are engaged at Dunsink I have found it convenient for some purposes to construct the four small tables here given. These tables show by inspection the corrections in tenths of a second of arc to the distance, D, and in whole minutes to the position-angle, *p*.

The distances in our measurements are generally under 300″. The tables only refer to zenith distances between 45° and 70°. Between 45° and the zenith the correction to the position-angle is presumed to be negligible and the correction to the distance may be taken at + 0″·1 per 300″. Below 70° of zenith distance more accurate calculations would be required than the tables can give.

TABLE I.

Refractions, Distance, and Position.

North Declination.

10°	20°	30°	40°	50°	60°	70°	80°	
1·0	2·6	3·6	4·3	5·1	5·9	7·0	10	
1·3	3·3	4·3	4·9	5·5	5·8	5·6	2·5	45°
2·0	3·2	4·1	4·9	5·8	6·6	8·2		
2·3	3·5	4·3	4·8	5·1	5·0	4·0		50
2·8	3·9	4·7	5·6	6·4	7·5	9·7		
2·9	3·8	4·3	4·7	4·7	4·3	2·5		55
3·5	4·5	5·3	6·1	7·2	8·7			
3·3	3·9	4·3	4·4	4·1	3·3			60
3·9	4·7	5·6	6·5	7·7	9·5			
3·6	3·9	4·2	4·2	3·8	2·4			63
4·2	5·1	6·0	7·0	8·2	11·0			
3·6	4·0	4·2	4·0	3·4	1·0			66
4·4	5·4	6·2	7·3	8·6				
3·6	4·0	4·0	3·7	3·0				68
4·6	5·6	6·5	7·6	9·0				
3·5	3·9	3·9	3·5	2·7				70

TABLE II.

Position-Angle – Parallactic Angle.

	360	350	340	330	320	310	300	290	280	270
	180	190	200	210	220	230	240	250	260	270
	180	170	160	150	140	130	120	110	100	90
	0	10	20	30	40	50	60	70	80	90
45°	2		2	1	1	1	1	1	1	1
50	2	2	2	2	2	1	1	1	1	1
55	3	2	2	2	2	2	1	1	1	1
60	3	3	3	3	2	2	1	1	1	1
63	4	4	4	3	3	2	2	1	1	1
66	5	5	5	4	3	3	2	1	1	1
68	6	6	5	5	4	3	2	1	1	1
70	7	7	6	5	4	3	2	2	1	1

Zenith Distance.

TABLE III.

Position-Angle.

	0	20	40	60	80	100	120	140	160	180
	180	200	220	240	260	280	300	320	340	360
−50	− V	−IV	− II	− I	0	0	−II	−III	− V	− V
−30	−IV	− V	−IV	−II	−I	0	0	− I	−III	−IV
−10	− II	−III	−III	−II	−I	+I	+II	+ I	0	− II
+10	+ II	0	− I	−II	−I	+I	+II	+III	+III	+ II
+30	+IV	+III	+ I	0	0	+I	+II	+IV	+ V	+IV
+50	+ V	+ V	+III	+ I	0	0	+ I	+ II	+IV	+ V

Parallactic Angle.

TABLE IV.

	0	I	II	III	IV	V
45°	0	0	0	1	1	1
50	0	0	1	1	1	1
55	0	0	1	1	2	2
60	0	1	1	2	2	3
63	0	1	1	2	3	4
66	0	1	2	3	4	5
68	0	1	2	4	5	6
70	0	1	3	5	6	7

Zenith Distance.

TABLE I. is for the purpose of finding an approximate value of the zenith distance, Z, and the parallactic angle, η.

Rule.—Enter I. with declination and hour angle in decimals of an hour; then Z is found along the horizontal line and η is found under the hour angle.

Example—Declension 50° and hour angle $8^{h}\cdot2$; then Z is 66° and η is 34°.

N.B.—η is to be taken + when the object is to the West of the meridian, and − on the East.

TABLE II. gives refraction for tenths of a second for a distance of 300″.

Rule.—Enter II. with Z and $p-\eta$, and obtain the additive correction in tenths of a second for a distance of 300″.

Example.—Z = 66°; $\eta = -34°$; $p = 296°$; then correction for refraction for a distance of 300″ is +0″·4, and for any distance D″ is $+0''\cdot4 \times D \div 300$.

TABLES III. and IV. give refraction in minutes of position-angle.

Rule.—Enter III. with η and p, and then IV. gives correction in minutes taking sign from III.

Example.—Z = 66°; $\eta = 30°$; $p = 140°$; correction for refraction = +4′: or Z = 63°; $\eta = 30°$; $p = 340°$; correction for refraction = −2′.

Errata.

Page 190, line 16, *for* xxxiii. *read* xxiii.
„ 196, „ 26, „ 1881 „ 1880.

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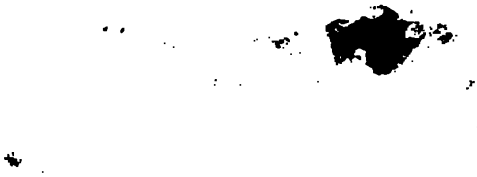

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